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THE TRANSFERENCE OF CONDITIONED EXCITATION AND CONDITIONED INHIBITION FROM ONE MUSCLE GROUP TO THE ANTAGONISTIC MUSCLE GROUP[1]

BY DELOS D. WICKENS

Ohio State University

The purpose of the experiment reported below is twofold in nature. In the main it is an investigation of the general nature of the conditioned response, and of the validity of the stimulus substitution theory of conditioning, while at the same time it is an attempt to strike another parallel between the behavior that arises in the conditioning situation and the phenomena common to conventional learning experiments. In particular, this study was undertaken to determine whether or not the phenomenon of transference is present in conditioning as well as in the orthodox learning experiments.

In one study in this field Gibson, Jack, and Raffel (4) conditioned their subjects to withdraw the middle finger of the right hand from an electrode at the sound of a buzzer. After this, the left hand was placed on the electrode and the buzzer sounded. Sixty-two percent of the subjects whom they had successfully conditioned responded by withdrawing their left middle finger although they had never received a

[1] This research was done as partial fulfillment of the doctoral degree at the University of North Carolina. The writer is indebted to Dr. J. F. Dashiell under whose direction this study was done, and also to Drs. A. G. Bayroff and Harry W. Crane for criticisms and suggestions.

shock in this situation. The experimenters then conclude that their results give evidence of bilateral transference of the conditioned response. To their conclusion, however, an important objection may be drawn. Numerous experimenters have pointed to the motor generalization that occurs in the early stages of conditioning, and there is no formal proof that this did not occur in the above experiment. It is just possible that no real bilateral transference of the conditioned response actually occurred, but what was assumed to be transference was only a part of the conditioned withdrawal response of the entire body, and that at one time the experimenters were measuring one part of the response and later a different part of it. The criticism seems particularly apt in that the hand merely rested on the electrode and was in no way restrained. Such a condition would make a withdrawal movement of the entire body as appropriate a response as lifting the finger.

To overcome this objection it was decided to perform an experiment in which the muscles to which the conditioned response might be transferred could not have been trained to make that particular withdrawal movement in the original conditioning. To satisfy this condition, antagonistic movements of the same finger were used. Specifically, an extension movement of the middle finger of the right hand was conditioned, and then tests were made to determine whether or not this conditioned response could be transferred to a flexor movement of the same finger. If any transference occurred in this situation it would be impossible to ascribe its cause to motor generalization.

Apparatus

The conditioning board proper consisted of a plank 28″ × 9″ × 1″. An adjustable elbow stop could be pushed against the subject's elbow to prevent him from making horizontal withdrawing movements with the whole arm, while a strap over the forearm prevented him from lifting the arm from the board. His hand was restrained by means of a padded wooden block which was screwed down across the back of the hand midway between the wrist joint and the first knuckles, or, in the case of the flexion conditioning, across the middle of the palm. The middle finger, which was the one to be conditioned, was isolated from the other digits by means of two brass strips rising at a steep angle from the board. These served to spread the other fingers

out much as in a finger ergograph, save in this case the other digits were not clamped. The middle finger then rested on an electrode at a place somewhere near the terminal joint. The electrode, with an exposure surface of about ⅜ square inches, consisted of a wire staple inclosed in a cigarette lighter wick and soldered to a piece of brass. The wick ran off to a small well of salt water, thus keeping the electrode damp throughout the experiment.

This electrode was bolted to a small bakelite platform which in turn was supported by a spring. As the spring offered little opposition to movement, and as the platform was supported in space by this spring only, it was possible for the subject to make slight downward movements as well as upward ones. A small bolt was screwed through the swinging bakelite platform so that when the platform was pushed downward the bolt made contact with a phosphor bronze strip, thus closing a circuit of a magnet on a polygraph, and so recording downward movements of the subject's finger. By adjusting the bolt it was possible to record extremely small movements of the finger on the part of the subject.

Upward responses of the finger were recorded by means of an aluminum strip, 4½ inches by 1½ inches, which was hinged to a brace directly in front of the subject's middle finger and rested lightly on the finger. When the finger was raised, this strip was brought into contact with a phosphor bronze brush which was soldered to an arm that was supported by a bolt. In this case, as in the above, it was possible to adjust one of the contacts so that slight movements could be recorded by another marker on the polygraph. The recorders for both upward and downward movements were in parallel with small 6-volt lights in the experimental room, making it easy for the experimenter to determine when either response was made. The other electrode applied to the subject consisted of a piece of phosphor bronze covered by heavy lamp wick with an exposure area of one square inch. This was first thoroughly dampened in salt water and then strapped with adhesive tape to the volar surface of the arm near the thumb joint. The essential features of the recording arrangements are shown in Diagram I.

The shock was produced by an inductorium, the primary of which was in parallel with a magnet on the polygraph as well as with a Cenco impulse counter, the latter making it easy to determine how many shocks had been given at any time during the experiment. At this point it might be said that the usual variability of the intensity of the shock when the inductorium was used did not seem to occur much in this experiment. The subjects were asked about the constancy of the shock after the experiment was over, and, with one exception, they reported no variability from stimulation to stimulation. The constant area of contact of one electrode, and the small, and thus relatively constant area of contact of the other, as well as the dampness of the electrodes, seems to have reduced this factor.

The conditioned stimulus was produced by a signal high frequency buzzer obtained from the Wholesale Radio Company. This buzzer was attached to the conditioning board to the right of the subject's hand, and was placed in parallel to a magnet on the polygraph. Both this and the inductorium were activated by the experimenter's pressing on separate push keys.

As the subject and the experimenter were in separate rooms it was necessary to use several signalling devices. A small 2⅓ volt light which was screwed to the wall directly in front of the subject's line of vision permitted the experimenter to signal the subject that his finger was out of position (*i.e.* making contact with upward response circuit) at a time when it should be on the electrode. By means of a telegraph circuit,

the push key for which was placed on the table on which the conditioning board rested, the subject could signal to the experimenter if he wished to do so for any reason.

The subject sat in a small room about 10′ × 6′ immediately adjoining the experimental room. In it was a book case that was covered by a white drape, an electric fan which served to ventilate the room as well as acting as a sound screen, and the table on which the conditioning board rested. There was one window in the room overlooking the campus. This was closed and the shade drawn during the experiment. The subject sat facing a door which communicated directly with the experimental room. During the experiment the subject could hear the hum of the polygraph motor and occasional noises from the halls of the building and from the campus, but whatever regular noises the experimenter made in performing the experiment were indistinguishable.

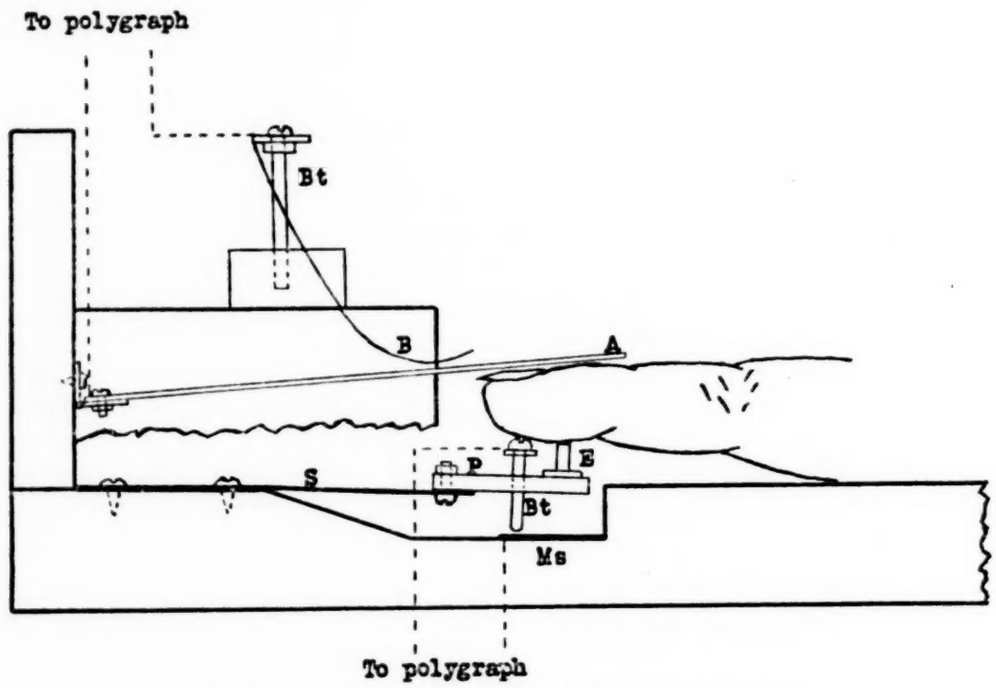

DIAGRAM I. Diagram of a cross section of the conditioning board showing the way in which the upward and downward responses were recorded.

A. Aluminum contact strip.
B. Contact brush.
S. Spring.
P. Bakelite platform.
E. Electrode.
Bt. Bolts for adjusting contacts.
Ms. Metal contact strip.

SUBJECTS

Thirty-two students in the elementary psychology class were used as subjects. Two of them were women. All of the subjects were familiar with the phenomenon of conditioning, and had seen a conditioning demonstration in the laboratory. The experiment they had seen was retraction of the whole forearm to shock conditioned to the sound of a bell.

EXPERIMENT I

Transference of Conditioned Excitation

Procedure.—After the subject had been strapped into the apparatus with his *palm down* so that an *extensor movement must be made to avoid the shock*, the recording contacts were adjusted so that movements of around $\frac{1}{16}$ of an inch in either an upward or downward direction could be recorded. The subjects were then told that this was to be a variation of the conditioning experiment that they had seen in the laboratory and that they were to attempt to take a passive attitude towards the experiment, neither responding voluntarily nor attempting to fight against becoming conditioned. The strength of the shock to be used was then determined by sliding up the secondary on the inductorium until the subject indicated that the shock was as strong as he was willing to take. The original adjustment of the strength of the shock was done with the door between the two rooms open and with the experimenter and the subject communicating directly with each other. In all cases the strength of the shock was increased somewhat as the experiment progressed. This was done to overcome any adaptation that might occur, and because the experimenter found that the subjects were willing to take shocks of greater strength than they would at first permit. Occasionally, the subjects even signalled to the experimenter that they wanted more shock, while a few subjects indicated by means of prearranged signals on the telegraph circuit that the shock had been stepped up too much.

Following this, the door between the two rooms was closed and the motor of the polygraph started. The buzzer alone was then sounded for three times.[2] Then the experimenter began to administer the shock and the buzzer together. These were given by the experimenter by hand, the shock key being manipulated by his left hand, the buzzer key by his right. The buzzer preceded the shock by a time interval of about half a second. The exact length of the interval was not, however, precisely determined, and as the stimuli were given by hand, this interval was also somewhat variable. This procedure was continued until the experimenter observed by means of the lights in parallel with the response circuit that the subject was beginning to anticipate the shock. After 4 or 5 consecutive anticipations had occurred, the experimenter began to test for the strength of the conditioned response. The strength was determined simply by finding the number of times the subject would respond to the buzzer alone before three consecutive failures to respond thusly.[3] After these failures had

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[2] Occasionally the subject reacted by lifting his finger the first time the buzzer sounded, and if such was the case the buzzer was sounded until three failures to respond occurred. These cases have been included in our results and should be considered in the same light as the other subjects for the following reasons. In the first place, these subjects reported that the high pitch of the buzzer startled them the first time they heard it and made them jump. Secondly, all the subjects in the experimental group who did this responded only once, and they did not respond to the buzzer again until a number of paired stimulations had been given them. Thirdly, there was no noticeable tendency for these subjects to behave differently either in terms of the ease of conditioning or in the tendency to transfer than did the subjects who did not respond immediately.

[3] This, of course, does not mean that the response was extinguished completely or near completely; it was only an arbitrarily chosen figure.

occurred the response was then reinforced one-tenth[4] of the number of paired stimulations it had been necessary to give up to that point.

The experimenter then entered the experimental room and changed the position of the subject's arm so that the *palm was up* and a *flexion movement now must be made in order to remove the finger from the electrode.* If necessary, contacts were readjusted so that slight movements would be recorded in this new position. From three to five minutes were usually consumed in this operation, and if the subject asked any questions about the experiment he was given a non-committal answer.

The door being closed and the polygraph motor started, the buzzer was then rung to test for transference. If the subject failed to respond for four consecutive buzzer stimulations the buzzer was paired with the shock until a fairly strong response had been developed. This was then measured as in the preceding situation. If he showed immediate transfer and began to respond immediately, the buzzer alone was given until three consecutive failures occurred. Following either of these occurrences, the response was then reinforced for the same number of trials as was used in the termination of the palm down situation—one-tenth the number used to establish the original response.

Following this, the hand was turned back to the original *palm down* position, the recording contacts adjusted if necessary, and tests were made to see if the original extension response was still present. If it was not, it was reinforced a few times until it was reëstablished.

In all, the entire procedure took about one hour and was accomplished in one sitting.

Control Procedure.—In view of the possibility that whatever transference may have occurred was the result of a general set to respond by lifting the finger to any stimulus because it is in a potentially painful position, or, as in Bernstein's experiment (2) to sensitization of the reflex as the result of many shocks, the following control was used with fourteen new subjects.

The subject was strapped into the apparatus with the palm down and the same general directions were given. After determining the desired strength of the shock, the door between the rooms was closed, the polygraph started, and the subject tested with the buzzer alone as in the experimental group. Following three consecutive failures to respond to the buzzer, the subjects were given 190 shocks *alone.* After this, the hand was turned over to the palm up position and the buzzer alone was sounded to determine if there was a tendency to respond. If there was not, paired stimulations were given until a conditioned response had been developed, the strength of which was measured in the same manner as in the experimental group. This terminated the procedure with this group.

At the completion of the experiment, the experimenter attempted to determine something about the significance of attitudes in this experiment by asking the subjects what they thought when their arm was turned over to the palm up position.

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[4] This figure was taken from the work of Hilgard and Marquis (9). They found this fraction approximately correct with the eyelid reflex in dogs for relearning after several months. Although our response, subjects, and time intervals are different, nevertheless the same figure was used. In this experiment it did not matter whether the strength of the response was the same as before the partial extinction; it was only important that it be at least that strong. There is considerable evidence in our results to show that the response was made even stronger.

Results of Experiment I.—The results for the 18 subjects in the first experiment are given in Table I. The heavy lines separate the three situations from each other. In the first column in each of the sections the plus or minus signs

TABLE I

SHOWING FOR EACH SITUATION THE NUMBER OF TRIALS NECESSARY TO ESTABLISH CONDITIONING, THE STRENGTH OF THE CONDITIONING, AND THE DEGREE OF TRANSFERENCE

Subjects	1st Palm Down Situation			Palm Up Situation			2nd Palm Down Situation		
	Buzzer Alone Test	Number of Reinforcements	Strength of CR	Buzzer Alone Test	Number of Reinforcements	Strength of CR	Buzzer Alone Test	Number of Reinforcements	Strength of CR
St.....	−	168	9	+	0	7	+	0	35
Wi.....	+	180	9	−	17	7	+	0	12
Hi.....	−	165	6	+	0	7	+	0	7
Ab.....	+	187	8	+	0	2	+	0	4
Mo.....	−	210	4	+	0	4	+	0	15
Ev.....	−	166	51	−	2	2	+	0	63+
Ca.....	−	300	4	−	8	4	+	0	4
Sa.....	+	311	5	+	0	48+	+	0	40
Fl.....	−	134	35+	−	5	54+	+	0	20+
Wl.....	−	126	45+	−	4	40+	+	0	12+
Ba.....	−	103	4	−	48	9	+	0	45+
Mu.....	+	90	22	+	0	11	+	0	27
Jo.....	−	35	8	*13*	23	13	*13*	4	6
El.....	+	87	65	+	0	17	+	0	94
Ga.....	+	207	46+	+	0	45+	+	0	35+
Sl.....	−	31	67+	+	0	30+	+	0	35+
Sk.....	+	150	8	−	10	5	+	0	18
Su.....	−	260	5	+	0	4	+	0	27

indicate whether or not a response was made to the sound of the buzzer without a shock having previously been given along with the sound in that particular situation.[5] In the second column of each of the situations are represented the number of paired stimulations that it was necessary to give in that particular situation to develop a conditioned response of the strength indicated by the number in the third column, the latter representing the number of conditioned responses that were given before three consecutive failures to respond.

[5] It should be remembered that if a plus sign appears in the first column of the first palm down situation, it means that the subject responded to the sound of the buzzer only on the first time it was sounded, whereas the meaning of the plus signs in the other columns is slightly different and will be explained later.

A plus in the first column of the second (palm up) situation may have either of two meanings. If there is a zero in the second column, it indicates that the subject responded to the buzzer alone the number of times shown in the third column before he failed to respond to it three consecutive times. If, however, there is a number greater than zero in the second column, the plus sign means that the subject gave only one response to the buzzer alone and that the response had to be reinforced in order to obtain a conditioned response of the strength indicated in the third column. Occasionally a plus sign will follow the number in the third column of the various situations. This indicates that, because of exigencies of time or for some other reason, the response was not extinguished to the usual point.

A few of the typical cases may be interpreted in the following manner. In the first palm down situation subject St did not respond to the buzzer alone when it was first sounded. After 168 paired stimulations he gave nine conditioned responses before he failed to respond to the sound of the buzzer alone for three consecutive times. When the hand was turned over he immediately responded seven times without being given any shocks, and, when the hand was turned back to the palm down position, he responded 35 times to the buzzer alone without any shocks being administered to him. Subject Wi responded the first time the buzzer was sounded but only the first time, and after 180 paired stimulations gave nine conditioned responses. When the hand was turned over, he failed to show immediate transfer, but after 17 reinforcements he gave seven conditioned responses. On returning to the palm down position, he gave 12 responses without reinforcements. The results for the other subjects are interpreted in the same manner.

Inspection of the table indicates a very definite tendency for the conditioned response to be carried over from one situation to the opposite. If we except Jo whose case will be discussed later, we find that ten out of the remaining 17 subjects showed an immediate transference of the response. The remaining subjects, with the possible exception of Ba,

developed strong responses in the new situation after a very small number of reinforcements.

Detailed Examination of the Exception.—The exceptional subject, Jo, offers an interesting contrast to the behavior shown by all the remaining subjects. When his arm was turned over so that a flexion movement had to be made in order to remove the finger from the electrode, he responded to the buzzer alone by continuing to make the extension response that had just been conditioned. This he did for 13 times before three consecutive failures occurred. Reinforcements were then given, and after 23 of them he had developed a flexion conditioned response with a strength of 13 responses. The hand was then turned back and again he exhibited the same phenomena. This time he made 13 flexor responses before extinction, although in this position the extension response—the one he had first learned—was the response that would remove his finger from the electrode. After four reinforcements he showed an extension conditioned response with a strength of six responses. In the language of the conventional learning experiments this subject showed both 'interference' and 'retroactive inhibition.' The former is shown by the fact that he responded to the same stimulus in a new situation by a movement that was appropriate to the old situation but not to the new one. In addition to this, when the response which was appropriate to this situation was first being reinforced, twice the subject made downward movements before the adaptive upward movement was made. This same behavior was evinced by several other subjects who did not show immediate transfer of the flexion response. The 'retroactive inhibition' is shown in this subject by the fact that the conditioning in the intermediate palm up flexor situation interfered with the previously established extensor conditioned response when tests were made for the presence of the latter response immediately following the flexor conditioning. Again, as in the previous situation, this subject made several maladaptive downward responses before the upward response was made when the extension response was being reinforced by the shock.

It is also possible that this subject evidenced some transference in that it took fewer trials to condition a stronger flexion response than it had taken to develop the first extension response (though again this might have been a case of sensitization of the reflex referred to above). Nevertheless, in view of the interference shown by this subject, it seems possible that a certain amount of interference may have been immanent in this experiment, and that a greater amount of transference would have been shown by the other subjects if such interference could by some magic means be removed. As it was, however, he was the only subject who made either flexion or extension movements to the buzzer alone when the opposed movement was the appropriate one.

The explanation of the behavior of this subject is difficult. Several suggestions were followed up but none of them proved fruitful. This subject had previously been conditioned to arm withdrawal from shock in the elementary laboratory, and to check this several other individuals who had previously been conditioned were asked to be subjects. These individuals behaved as did all the other subjects and have been included in the results of the experimental group. Secondly, this subject was a track man, and it seemed barely possible that the track training he had received caused him to be highly aware of the specific muscular response he was making. At the time it was possible to obtain only one other track man as a subject, and he reacted as did all the others. As yet the writer is still in the dark as to how it is possible to produce this phenomenon by experimental means and as to what factors contribute to it.

Control Results.—As we have already mentioned above, it is possible that the immediate appearance of flexor movements in the palm up situation might be due (*a*) to sensitization as a result of the many shocks given to the muscles involved in moving the middle finger, or (*b*) to the presence of a general set to respond by lifting the finger to any stimulus as a result of the fact that the finger is in what the subject has learned is a potentially painful position. If either or both of them were the cause or causes of the transfer, then we would

expect that 190 shocks, given alone and unaccompanied by the buzzer,—a number chosen because it was usually more than enough to establish extensor conditioning in our experimental subjects—would serve to sensitize the muscles, or to establish the above mentioned set, and thus the subject would immediately respond to the sound of the buzzer when the hand was turned over to the palm up position. Such however, is not the case as can readily be seen by an examination of Table II.

TABLE II

SHOWING THE NUMBER OF SHOCKS ALONE GIVEN, THE NUMBER OF TRIALS NECESSARY TO ESTABLISH CONDITIONING, AND THE DEGREE OF CONDITIONING

Subjects	Palm Down Situation		Palm Up Situation		
	Test	Shock Alone	Buzzer Alone Test	Number of Reinforcements	Strength of CR
Co.	−	190	−	79	3
In.	−	190	−	35	136+
Cu.	−	190	−	112	7
Au.	−	190	−	73	4
Ri.	−	190	−	71	3
Bk.	−	190	−	146	5
Tu.	−	190	−	73	155
Bl.	+	190	−	150	3
Gr.	−	190	−	45	8
Sh.	+	190	+	0	2
Sm.	−	190	+	0	6
Br.	−	190	−	98	9
Th.	+	190	−	107	9
Sa.	+	190	−	93	5

Here again the heavy lines mark off the different situations, the marks in the first column in each section indicating whether or not a response was made before any shock was given. The second and third columns of the palm up situation indicate as in the other table the number of reinforcements necessary to establish a response of the specified strength. It will be seen that only two of these 14 subjects responded to the buzzer alone when it was first sounded in the palm up situation.

The Exceptions.—The case of one of the control subjects, Sh, who immediately responded to the buzzer in the palm up position is an interesting one. In the initial test when the

buzzer alone was sounded in the palm down situation, she was the only one of all the subjects to respond more than once, for she immediately gave three consecutive responses—more than she later gave in the palm up situation. In this latter position she failed to respond the first time the buzzer was sounded, responded twice in succession, then failed for three consecutive times. Indeed, the conditioning did not seem as strong after the shocks had been administered as it did before. As a further check, the experimenter reinforced the response for three times and was able to obtain a conditioned response with a strength of greater than 45 responses. The hand was then turned back to the palm down position, and an immediate transfer of a response of greater than 35 responses was obtained. It does not seem, then, that this atypical case may be used to throw doubt on the reality of the transfer of the conditioned response. The other control subject, Sm, who showed immediate transfer, acted quite as the other subjects did, and he offers some contradiction to the general findings of the experiment.

Interpretation.—Since in general the control group does not show a tendency to respond to the buzzer alone when it is first sounded in the palm up situation, the transference shown in the experimental group can hardly be the result of reflex sensitization, or of the establishment of a general set to withdraw the finger at the incidence of any stimulus. On the contrary, it can only be explained as the result of some connection between the sound of the buzzer and a general defense movement, this connection between the two having been established as the result of previous repeated pairings of the sound of the buzzer with the shock.

Summary.—In conclusion, then, we may state that the results of the experiment very definitely indicate that the conditioning of a response of one muscle group can be immediately transferred to an opposed movement if the situation is such as to make this opposed movement a more appropriate and adaptive response. The experiment also indicates that this is not a rule without exceptions, but that in some cases interference may be strong enough to mask what transference

might be present and this despite the appropriateness of the transferred response.

Experiment II

Transference of Conditioned Inhibition

If it is possible to obtain a transference of an active conditioned response, it is logical to assume that we could obtain a transference of conditioned inhibition as well. Since in all cases except one, the extension response was found present when the hand was put in the palm down position following the test for transference of conditioning of the flexor response, this offered a good situation in which to test for transference of conditioned inhibition. For this reason a number of the experimental subjects were called back for a second sitting and the following experiment was performed.

Procedure.—The arm was strapped in the apparatus with the palm down, and the subjects were reconditioned to approximately the same strength as they had shown during the first session. The response, partially extinguished while measuring its strength, was then reinforced one tenth of the number of trials used in the original conditioning. The hand was then turned over and the tests were made for transfer of the conditioned response as before. If the response was not present, reinforcements were given until it had become fairly strong. At this point the procedure differed from the previous in that instead of reinforcing after three consecutive failures to respond, the buzzer continued to be given until the response was extinguished to the point of 15 consecutive failures to respond to its sound. The arm was then turned back to the palm down position, and the buzzer alone was sounded to determine whether or not the extensor response had been extinguished along with the other. If it was present its strength was measured as in the previous experiment.

Results.—The results for the six subjects used in this experiment are given in Table III. The first two sections are to be read in the same way as those sections in Table I. The subjects have been given the same letters as had been given them in the first experimental group, and by reference back to Table I it can be seen that the subjects were reconditioned to something close to the strength of the original conditioning. In the second palm down situation of Table III, the first column refers to the immediate strength of the extension response in this situation, that is, after the flexion response had been extinguished to the extent of 15 consecutive failures to respond to the sound of the buzzer alone. The

second column refers to the immediate strength of the response in the first experiment when no extinction of the flexor response had been attempted. Comparison of these two columns shows that in the extinction experiment the extensor response was either extinguished completely, or its strength was greatly diminished from what it had been in the first experiment. Since in our first experiment on these same subjects, the extensor response was always present when tested the second time, we must conclude that the presence of the response in one case and the absence of it in the other

TABLE III

SHOWING THE NUMBER OF TRIALS NECESSARY FOR RECONDITIONING, THE STRENGTH OF THE CONDITIONED RESPONSE, THE DEGREE OF EXTINCTION, AND THE TRANSFERENCE OF CONDITIONED INHIBITION

Subjects	1st Palm Down Situation			Palm Up Situation				2nd Palm Down Situation	
	Buzzer Alone Test	Number of Reinforcements	Strength of CR	Buzzer Alone Test	Number of Reinforcements	Strength of CR	Degree of Extinction	Present Strength	Strength in Experiment I
Wi.....	+	14	7	+	0	3	15	0	12
Ab.....	+	144	9	+	0	14	15	1	4
Mo.....	+	43	5	+	0	3	15	1	15
Mu.....	+	15	21	+	0	41	15	1	27
Jo.....	−	10	5	*3*	3	4	15	0	?
Sk.....	+	120	4	−	10	7	15	0	18

is due solely to the one difference in our experimental procedure, namely, that in the first experimental situation the flexor response was reinforced, and in the second experiment it was extinguished. Very obviously this means that conditioned inhibition can be transferred just as conditioned excitation can be.

Subject Jo is again interesting. As in the previous experiment he responded in the situation where the flexor response was the appropriate one by making extensor responses to the buzzer alone during the first three times it was sounded. It was then necessary to reinforce the flexor response three times before a conditioned response with the strength of four was obtained. When the hand was turned back to the

palm down position after extinction of the flexor response, the extensor response, however, was also extinguished.

There are several possible interpretations of this behavior. Since we have no proof that this subject would have carried over the extensor response through the intermediate procedure of flexor conditioning, it is possible that the intermediate procedure had no effect on the extensor response but that this response had waned of its own accord. To this interpretation the following objection may be raised. It assumes that the extensor conditioned response would have disappeared merely through the lapse of time—about 15 minutes—required to perform the palm up procedure. Since this never occurred in any of the other subjects in the first experimental group, it does not seem to be a likely explanation, and is, therefore, rejected.

The second interpretation that may be made of the results of this subject is that there is real transference of experimental inhibition. Were the two situations completely isolated from each other, we should have expected that the extinction of the flexor response would have no effect on the extensor response, but this does not seem to be the case. If the objection raised to the interpretation in the above paragraph is valid, and this seems the only other possible interpretation of the results, then we must conclude that the inhibitory response has been transferred. If we accept this conclusion, then we lend more weight to the assumption that for this subject transference was present in the first experiment, but that this transference was greatly masked by the presence of a large amount of interference; for it is not likely that inhibition would be transferred and the active response would not. There seems little justification for the notion that at one time the systems will be completely isolated and at another time they will not.

Summary.—The results of this experiment are in complete accord with those of our first. The data have shown that there is a transference of conditioned inhibition from one muscle group to its antagonist just as there is a transference of conditioned excitation from one muscle group to its

antagonist. The results are the same as what we might logically assume to be the case from our knowledge of the first experiment, and it adds further weight to the findings of the first experiment.

Conclusions and Discussion

Parallels between Learning and Conditioning.—This experiment has been successful in eliminating a possible artifact that might have been supposed to be a cause of the transfer in the work of Gibson, Jack, and Raffel (4). At the same time it has supported the general results of this experiment and lines itself up with several other approaches to the same problem. Thus Gibson and Hudson (3) have conditioned the knee jerk of the right leg to the sound of buzzer, flash of light, and to kinesthetic stimuli arising from pressing a hand dynamometer, and were able to obtain transference of this response from one leg to the other. Similarly Bechterev (1) reports that if a dog is unable to make a response of the leg that has been conditioned it will respond with the leg on the other side. The phenomenon, then, of transference has been shown by these experiments to be present in several typical conditioned response situations just as it is present in many of the usual types of learning situations.

The experiment, however, goes a bit further than do those mentioned above and has identified several other learning phenomenon in the conditioning situation, for the behavior of the anomalous Jo gives evidence for both interference and retroactive inhibition.

Theoretical Considerations.—In addition to affirming one, and adding two more to the parallels between conditioning and learning, this experiment presents some rather formidable evidence against one type of conditioned response theory, namely that of stimulus-substitution. If we may take Guthrie's hypothesis as fairly representative of this type of theory and attempt to use it to explain our results we may see how inadequate it is. The essence of the theory is stated in the following sentence: "A combination of stimuli which has accompanied a movement will on its recurrence tend to

be followed by that movement" (**5**, p. 26). Now in the palm down situation of our experiment the sound of the buzzer was usually accompanied by an extensor movement of the hand. It is this movement, then, that we should expect would follow the sound of the buzzer, and reasoning from this theory, we should predict that all our results would follow the same pattern as our single atypical subject did. The results are, of course, contradictory to the prediction, and the great weight of evidence is against the concept that one stimulus, the sound of the buzzer in our case, acts as a substitute to produce the same response as did the stimulus that followed it in time—the shock applied to the pad of the middle finger. We must conclude that this simple reflex-arc type of explanation of behavior in the conditioning situation should be dismissed as a hypothesis that does not fit the experimental facts.

In view of the inadequacy of this simple concept of conditioning to account for the behavior that arises in a typical conditioning experiment, it seems unfair to define the conditioned response as narrowly as is done by Tolman (**14**). Such a revision to the meaning of the conditioned response serves to weaken his position, for his substitution of sign-gestalt learning was justified so long as conditioning was conceived of simply as a case of stimulus-substitution, but since the nature of the conditioned response should be defined by the kind of behavior that arises in typical conditioning experiments, rather than by an erroneous and over simplified explanation of that behavior, and as this and other experiments have shown the conditioning behavior to be far different from what the usual diagrams would lead us to believe, it is highly possible that conditioning may explain much that Tolman considers can be understood only in the light of the sign-gestalt theory of learning. It seems, then, that in many cases whether we wish to interpret conditioning as sign-gestalt learning, or sign-gestalt learning as conditioning depends entirely on the bias of the interpreter.

There are several existing theories of conditioning that admirably explain the results of this experiment. Razran

(12) has developed a phylogenetic and ontogenetic theory of conditioning that is based on attitudes. Essentially it is that during the conditioning of a consciously reportable and consciously controllable response, the subject assumes a certain attitude or attitudes toward the situation. These attitudes, according to Razran, are best conceived of as verbal organizations or postural sets; but whatever they are, they are functionally central, and they are dominant over the mere linkages or associations of the situations presented. As a result of them, depending upon whether the attitude is positive ("the experimenter wants me, the subject, to do *b* when I hear *a*"), negative ("he can't fool me again, or I must not do *b* when I hear *a*") or indifferent (a maintenance of the status quo, a spectator curiosity, or attention directed to things other than the experiment), the conditioning may or may not be obtained, or, if the attitude is indifferent, the response may be highly unstable when it is obtained. Again, the stability of the response and the presence or absence of experimental extinction depends on the kind of attitude the subject holds. By accepting this theory the explanation of the results of this experiment apparently becomes quite simple, and we can explain cases of transference as well as the perseveration of the old response in a new situation merely by assuming that the behavior is the result of the subject's attitude.

The simplicity of this explanation, however, is only apparent, and its validity is questionable; for if the data from various parts of this experiment are considered more closely, we find that the theory either becomes hopelessly complex or lacks direct supporting evidence. When the subjects of this experiment were asked what they thought when the arm was turned over to the palm up position following the extensor conditioning, the usual reply was that they didn't think of anything. They, of course, must have known that now they must make just the opposite movement to lift the finger from the electrode, but none of the subjects decided—if we may rely on verbal reports taken some 15 or 20 minutes later—that this was the movement they would make. The finger

just flew up without the subject thinking about it, or often despite the fact that he was consciously trying to inhibit the response to the buzzer alone. The attitudes in this experiment are, then, far too elusive to have much explanatory value.

A case in point is one subject with whom the experimenter was unsuccessful in extinguishing the response during the last palm down situation. The experimenter informed the subject that no more shocks would be given, and then even bet the subject a small amount that he could not inhibit his responses for the next ten stimulations of the buzzer. Much to the subject's disgust the experimenter won the bet. Now in this case we have on the part of the subject a strong conscious attitude against responding, but it seems to avail him little. Either the theory fails completely to account for these results and must be discarded or we must introduce a prop and assume that the subject had a stronger unconscious attitude—granted there be such a thing—toward responding than conscious attitude against responding, and in this manner explain our results.

It is for the above reasons that this theory must be rejected as an explanation of conditioning, for it is far too slippery to be of any real value. If conscious attitudes fail to explain the results, then we seem to have to call upon unconscious attitudes to relieve us of our embarrassment. The theory is too invulnerable of criticisms and it seems to be of such a form that it would be impossible to put it to a crucial test. As a scientific theory it lacks another important attribute, that of predictability. Even though we might know and be able to control all the external characteristics of our situation, we would, in the case of human subjects, be totally unable to determine in advance what the subject might do, for this would depend entirely on his attitude, conscious or otherwise.[6] True, if the subject told us his

[6] This implies, of course, that the attitude cannot be controlled, and it is only fair to state that in a more recent study Razran (13) has obtained a consistent conditioning that he assumes to be the result of holding the attitudes constant. The experiment, however, brings up the ticklish problem of determining what an attitude is, and it is doubtful that at the present state of our knowledge the phenomena of the

attitude we might predict, but what of those cases when the attitude is negative but when conditioning occurs?

The theory advanced by Gibson, Jack, and Raffel (4) to account for the results of their first experiment is fairly satisfactory for this experiment. They hold that the adult has organized habits of withdrawing from painful stimuli, and that the conditioned stimulus has become effective for the entire repertory of avoiding reactions. As a result, the member of the body that is in a potentially painful position is in a state of readiness, or sub-activation to make the appropriate withdrawal movement. This theory, however, does not stress sufficiently the relationship between the incidental stimulus and the painful stimulus, a condition which is most obviously seen to be present when the results of our control and first experimental group are compared; for we would predict from this theory that the control group would show just as much tendency to respond when the buzzer was sounded in the palm up situation as did the experimental group, since both groups of subjects were aware that the finger was in a potentially painful position. If putting the finger in a potentially painful position serves to subactivate that finger, it might be readily expected that with both groups any stimulus—the buzzer for example—would trip off that response. The theory also becomes somewhat awkward when we try to apply it to the results of our experimental extinction group. Here it may be necessary to conceive of the subject as being in a state of unreadiness to respond with the appropriate defense movement despite the fact that this position has always been the painful one. This points to the difficulty that this theory would have in the explanation of experimental extinction, for it seems to work in one way only.

In addition to these objections, there is one which Gibson and Hudson (3) point out in their experiment on bilateral transfer of the knee-jerk. In this case they state it is difficult

conditioned response can be clarified by the introduction of an even more complex type of behavior, unless it be that the conditioned response *is* complex because it must be subsumed under that *more* complex behavior.

to conceive of a generalized habit of kicking, and such must be the case if their original theory is to be applied to this later experiment. Since this theory then applies only in a limited way to defense movements, and falls down when applied to other sorts of conditioned responses, it is much too narrow in scope to be accepted as an explanation of the conditioned response even in the fields in which it does apply. It is far better to withhold speculation until enough data has been gathered to determine whether one generalized theory will fit all conditioning, or whether we must develop specialized theories for different kinds of conditioning.

In view of the fact that none of the theories examined have been adequate to account either for the results of this experiment or other common phenomena of conditioning, or both, and because of the present lack of experimental material bearing upon a number of important aspects of the conditioned response, it seems that it is foolhardy to attempt to advance a theory that will explain this experiment and the various other characteristics of conditioned responses.

We may consider, then, that this experiment merely adds something more to our knowledge of the nature of the conditioned response, and it points to this type of behavior as being of considerably more importance from a biological point of view than the stimulus-substitution formulation of it would lead us to believe. This, along with several other experiments, emphasizes the adaptive nature of the conditioned response, an emphasis that has already been made by Liddell when he stated that "we may think of the conditional motor reflex in anticipation of the electric shock as an addition to the animal's defensive equipment. To be adequate, the reflex must fulfill the purpose for which it is established. That purpose is the protection of the skin against noxious stimulation. When warned of an approaching electric shock the 'conditioned' animal seeks to defend itself in a manner *appropriate to the situation*" (**10**, pp. 54–55). Such a view of the conditioned response is in harmony with experimental results, and it may be thought of not as a theory of conditioning, but merely as a description of the organism's behavior

in certain conditioning situations. As a general formulation it should aid us in predicting what the organism will do without attempting to explain why he does so.

Summary

To investigate the possibility of transference of the conditioned response, antagonistic responses of the middle finger of the right hand were studied. After an extensor conditioned response was established, the hand was turned over and tests were made for the transference to this response to a flexor movement. A second experiment was concerned with the transfer of conditioned inhibition. In this case the flexor response was extinguished and tests were made to determine if a previously formed extensor response was also extinguished. The results show:

1. Transference of conditioned excitation from one muscle group to the antagonistic muscle group does occur in most cases.

2. Transference of conditioned inhibition from one muscle group to the antagonistic muscle group is also present.

3. Interference may occasionally be sufficiently strong to mask the transference that is present.

4. Retroactive inhibition may occur in conditioning just as it does in certain typical learning situations.

5. No existing theory of conditioning is adequate to account for transference and other phenomena of conditioning or both.

References

1. Bekhterev, V. M., *General Principles of Human Reflexology*. New York: International, 1933.
2. Bernstein, A. L., Temporal factors in the formation of conditioned eyelid reactions in human subjects, *J. Gener. Psychol.*, 1934, **10**, 173–197.
3. Gibson, J. J., and Hudson, L., Bilateral transfer of the conditioned knee-jerk, J. Exper. Psychol., 1935, **18**, 774–783.
4. Gibson, J. J., Jack, E. G., and Raffel, G., Bilateral transfer of conditioned responses in human subjects, J. Exper. Psychol., 1932, **15**, 416–421.
5. Guthrie, E. R., *The Psychology of Learning*. New York: Harpers, 1935.
6. Hilgard, E. R., The nature of the conditioned response: I. The case for and against stimulus-substitution. *Psychol. Rev.*, 1936, **43**, 366–385.
7. Hilgard, E. R., The nature of the conditioned response: II. Alternatives to stimulus-substitution, *Psychol. Rev.*, 1936, **43**, 547–564.

8. HILGARD, E. R., The relationship between the conditioned response and conventional learning experiments, *Psychol. Bull.*, 1937, **34**, 61–102.
9. HILGARD, E. R., AND MARQUIS, D. G., Acquisition, extinction, and retention of conditioned lid responses to light in dogs, *J. Comp. Psychol.*, 1935, **19**, 29–58.
10. LIDDELL, H. S., The conditioned reflex. In *Comparative Psychology*, F. A. Moss (Ed.). New York: Prentice-Hall, 1934. Pp. 247–296.
11. LIDDELL, H. S., JAMES, W. T., AND ANDERSON, O. D., The comparative physiology of the conditioned motor reflex based on experiments with the pig, dog, sheep, goat, and rabbit, *Comp. Psychol. Monog.*, 1934, **11**, No. 51.
12. RAZRAN, G. H. S., Conditioned responses: An experimental study and a theoretical analysis, *Arch. of Psychol.*, 1935, **28**, No. 191.
13. RAZRAN, G. H. S., Attitudinal control of human conditioning, *J. Psychol.*, 1936, **2**, 327–337.
14. TOLMAN, E. C., Theories of learning. In *Comparative Psychology*, F. A. Moss (Ed.). New York: Prentice-Hall, Inc., 1934. Pp. 367–408.

THE WORK DECREMENT AS AFFECTED BY THREE KINDS OF MEANINGFULNESS[1]

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Interest in repetition as a cause of decrement in mental work has been manifested for a number of years. Dodge (**6**) called attention to its importance in 1917 in his *Laws of Relative Fatigue*, and several experimental investigations have been devoted to it.

Thorndike (**20**) noted that subjects who were permitted a choice in their responses of numbers from 0 to 9 showed a rather surprising avoidance of immediate repetition. Telford in his later elaboration of the same experiment obtained similar results.

Poffenberger (**15**) found that the four tasks, continuous addition, judgment of compositions, completion of sentences, and intelligence tests, were productive of decrement in the order named. He attributed the decrement to the continuous exercise of a single function but remarked that opportunity for frequent short rests in the course of work may prevent decrement.

In 1926, Robinson and Bills (**16**) reported their rigidly quantitative study of the effect of repetition. Requiring their subjects to write continuously such letter sequences as *ababab*, they investigated five degrees of homogeneity-heterogeneity. As judged by either the first minute or the tenth minute of the work period, the results under the five conditions are strikingly clear—the greater the degree of homogeneity and hence of repetition, the greater the work decrement. Similar results were obtained with letter naming.

In marked contrast to these quantitative investigations are the Gestalt studies of fatigue. Karsten, working under

[1] From the Psychological Laboratory of the University of Chicago. The writer is greatly indebted to Dr. Arthur G. Bills, who directed the research.

Lewin (**12**), pointed out the limitations of the principle of repetition. She showed that it is not always a cause of mental fatigue, since one child voluntarily opened and shut a door eighty-two times. Moreover, after 'psychic satiation' had occurred in performing such tasks as making continuous strokes with a pencil in the rhythm 3–5 (/// /////), repetitions of the unit were still possible if the unit were changed to 4–4 (//// ////).

Lewin (**10**) does not offer an alternative principle to repetition as productive of mental fatigue or boredom, but Hoche (**8**) suggests that the chief cause of boredom (Langeweile) in any given period is poverty of the content offered.

No other variable in the work materials has been studied with the thoroughness accorded to repetition.

Arai (**1**) and Painter (**14**) were interested in difficulty, but since Arai used only one grade of difficulty in each section of her experiment, and since Painter's performance broke down before other grades were tried, their results regarding difficulty as a principle of decrement are not clean-cut.

Much clearer is the evidence regarding the influence of competition among the required responses as found by Robinson and Bills (**16**) in their study of finger movements in a typewriting task. They found that the use of two fingers alternately or of three in turn was productive of more decrement than the use of one finger in repeated movements. But since the competition belongs to the responses rather than to the stimuli, the second principle of the work decrement is not strictly a second principle of variation in the materials.

For the past few years, investigators of mental work have centered their attention upon problems other than the effect of variations of material. Hence the principle of repetition stands almost alone, though in 1926 Robinson and Bills (**16**) ended their paper, "Two Factors of the Work Decrement," with the statement: "Other principles are also surely fundamental in the determination of the decrement, and all of them offer rich territory for exploration."

Problem

It is the purpose of the present study to select variables other than repetition, to prepare materials embodying them, and to investigate the effect of the variation upon the course of mental work.

Of the many variables that are doubtless available, three have been chosen: connectedness, associative value, and comprehensibility. All three are familiar. Cattell (**5**) reported in 1886 that he could see and name words more rapidly when they made up sentences than when they were written separately; McGeoch (**13**) found that the associative value of nonsense syllables facilitated learning them; and Gates (**7**) and many others in discussing reading tests have emphasized the importance of accurately measuring comprehension of materials.

In order to be suitable for this experiment, the materials embodying the variables must meet three requirements: (1) they must be capable of two or more degrees of variation; (2) they must not vary as regards the amount of repetition involved, that is, the homogeneity-heterogeneity factor must be held constant; (3) they must be of such nature that responses can be made and recorded at comparable rates throughout the work period. The materials used will be described in detail for each experiment.

Apparatus

The apparatus was an electrically driven kymograph, furnished with two telegraph keys connected with metal points that marked on a moving waxed paper tape. The subject responded vocally. The experimenter pressed one of the keys at each response, and an assistant recorded errors with the other key.

Procedure

The procedure was kept highly uniform. The subject sat with his back to the apparatus and to the experimenters. The same general directions were always given: "This is a test of both speed and accuracy. You should try to do two things: (1) read the selection in as short a time as possible, (2) make as few mistakes as possible. If you make a mistake, go right on."

The amount set was employed consistently. Bills and Brown (**3**) found it distinctly more efficient than the time set, and it would seem to have special advantages in studies of varied materials. The criteria were speed and accuracy.

Equalization of practice was secured by having a preliminary period in each experiment in which all the materials to be used later were given equal representation, and by employing all the permutations of temporal order an equal number of times. In order to differentiate practice effects from fatigue effects, Bills's simplification (2) of the Thorndike (20) method was employed. Each day's task was made up of a certain number of equal divisions or units. The last unit for the day presumably combines the maximum practice effect with the maximum fatigue effect (complicated, of course, under the amount set with end spurt). A repetition of the unit the next day presumably represents the maximum practice effect with the fatigue eliminated. The difference between them is taken as the work decrement.

In order to ascertain the significance of the decrements, the formula for correlated variables was employed throughout the experiments:

$$\sigma_D = \sqrt{(\sigma_{av.1})^2 + (\sigma_{av.2})^2 - 2r\sigma_{av.1}\sigma_{av.2}}.$$

To be reliable, a difference must exceed its standard error by a ratio of 2.7 or more.

EXPERIMENT I

Connectedness

The first and most extensive experiment concerned the effect of varying degree of connectedness upon the course of mental work.

Materials

A narrative was chosen as representing the highest degree of connectedness. It was broken up into sentences arranged in scrambled order for the second degree and into words in scrambled order for the lowest degree. In order to enable the responses to proceed at comparable rates, all the words were monosyllables of three letters each. They were written backward. It was the subject's task to reverse mentally the letters of each word and to pronounce the word aloud. A sample sentence will make clearer the nature of the task: "eht yad saw toh." This, of course, was to be read, "The day was hot." No capital letters or marks of punctuation were used, but the word *dot* (*tod*) was placed at the end of each sentence, so that the task Sentences would not involve special confusion. The twelve-hundred-word narrative was made as melodramatic as three-letter words and the writer's limitations would permit.

For the second day's task a sequel to the narrative was prepared, which was broken up in like fashion into sentences and into words for the other grades of material.

Procedure

The conditions of the experiment demanded five days of work of each subject. These were the days of the school week, Monday through Friday. Monday was always devoted to practice, which was divided into two parts, a short preliminary period in which directions were given and words and sentences were tried out, and a longer period in which all the vocabulary to appear later in the regular tasks was presented, partly as separate words, partly in sentences, partly in narrative form (though not as given later). The materials for practice, as well as those for the other tasks, Words, Sentences, and Narrative, were divided into eight units of 150 words

each. The end of the unit was not apparent to the subject, since no break appeared in the work, but it was marked on the tape by the assistant who recorded errors. The next day at the beginning of the work period, two more units of the task were given. This work was called, for convenience, the *Sequel*. A typical schedule for a week is as follows:

Monday	Tuesday	Wednesday	Thursday	Friday
Preliminary practice Practice	Practice sequel Words	Words sequel Sentences	Sentences sequel Narrative	Narrative sequel

Since there were three kinds of tasks, there were six permutations of the temporal order. Ninety-six subjects made possible sixteen repetitions of each order. They were junior college students who were not aware of the purpose of the experiment.

Results

Figures 1 and 2 show the work curves for speed and accuracy respectively.

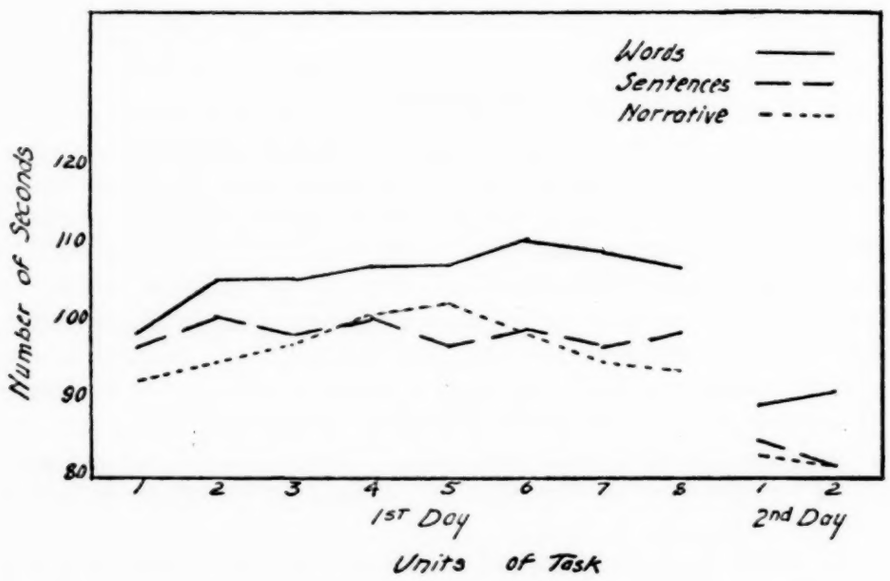

FIG. 1. Average time in seconds per unit of task for the three degrees of connectedness.

The chief interest in the experiment lies in the decrements, which are measured by the difference between the last unit of the first day's task and the first unit given on the succeeding day. All three grades of material show considerable decrement in speed. In the case of Words, it is 16.85 sec. with a standard error of 1.12; with Sentences, it is 14.52 ± .83; and with Narrative, 11.09 ± 1.07. The difference between the

decrements of Words and Sentences is 2.33 ± .56. Words and Narrative show a difference of 5.76 ± 1.46, and Sentences and Narrative, a difference of 3.43 ± 1.20. These differences are all significant, the critical ratios being 4.2, 3.9, and 2.9, respectively.

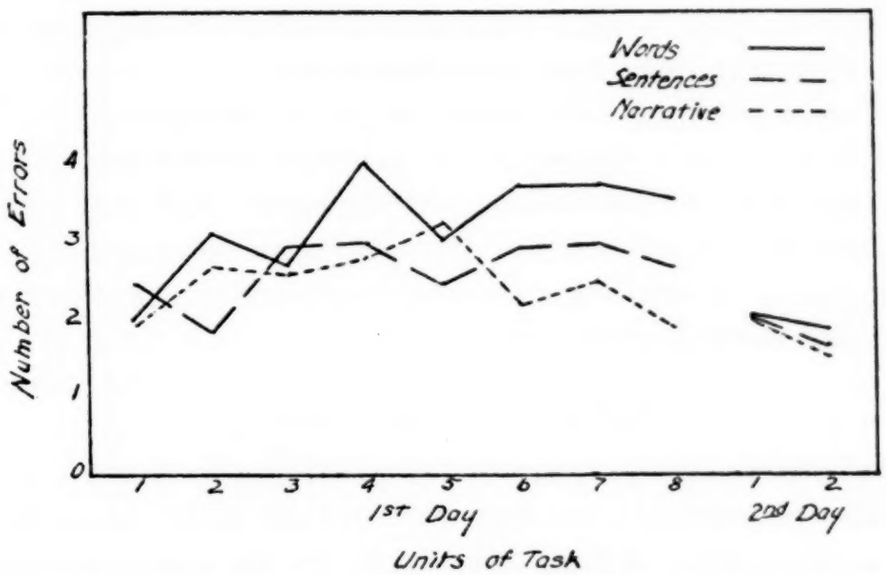

FIG. 2. Average number of errors per unit of task for the three degrees of connectedness.

With the criterion of accuracy, there is a huge decrement for Words, 1.21 ± .161; for Sentences one about half that size, .60 ± .23; and for Narrative, none at all. The differences between the decrements are not very large for Words and Sentences (.62 ± .35) or for Sentences and Narrative (.69 ± .32), but the extremes of connectedness, Words and Narrative, have a considerable difference (1.30 ± .39), which is highly reliable.

Results under the two criteria are more easily compared when both are reduced to percentages. The score for decrement in each case is divided by the score for the period of the greatest efficiency, that is, the first unit of the second day's task. Speed shows a decrement of 18.8 percent with Words, 17.3 percent with Sentences, and 13.4 percent with Narrative. The decrements for accuracy are more striking—51.9 percent with Words, 28.8 percent with Sentences, and none with Narrative.

Materials of the three degrees of connectedness show differences not only in the decrements but also in the levels of work efficiency, in spite of the fact that all three tasks are constructed of precisely the same words. An average of 106.07 sec. was required for each unit of Words, while 98.59 sec. sufficed for Sentences, and 96.80 sec. for Narrative. An average of 3.26 errors was made with Words, 2.67 with Sentences, and only 2.49 with Narrative.

How are these differences to be accounted for? Is it possible that with disconnected material irrelevant activities interrupt work and produce a decrement? And is it possible with connected materials that these activities are prevented from arising or are suppressed by an activity that forwards work and reduces decrement?

Subjects' General Behavior

No introspections were required of the subjects. A few of their spontaneous remarks were jotted down, however, as well as occasional observations made by the experimenter.

With the task Words, general restlessness was often noted. F. B. remarked, "I couldn't keep my mind on it today." Many errors were made with *was* and *saw*. C. R. kicked the wall during the last half of the work. The assistants reported that J. P. kicked the wall and swore.

With Sentences, also, the subjects often confused *was* and *saw*. There was more restlessness in the early part of the task than in the latter part. F. M. said, "I couldn't get the drift of some of this."

With Narrative, several subjects blocked before *ebb* and *lea*. S. R. laughed repeatedly. M. J. (one of the fastest workers) read 'with expression.' C. E. (one of the slowest workers) paused at the ends of sentences.

Conclusions

1. The task of reading three-letter words written backward is productive of reliable decrements with both criteria and with all degrees of connectedness, except that accuracy shows none with Narrative.

2. As judged by the criteria of speed and accuracy, connectedness of materials bears an inverse relationship to the decrement developed in the task.

3. With both criteria, connectedness results in three distinct work levels, the level of least efficiency occurring with discrete materials, the level of greatest efficiency, with the most highly connected materials.

4. In general, connectedness affects the activities that occur in mental work in such a way as to facilitate work and to reduce decrement.

Two questions seem to demand immediate investigation: (1) With discrete materials, may mental work be impeded and the decrement increased by irrelevant activities permitted or stimulated by the work itself? In other words, will variations in the associative value of the separate stimuli affect the course of mental work? (2) With connected materials, is it possible to discover the nature of the facilitating activity? Is it dependent in any way upon comprehensibility?

Experiment II

Associative Value

The effect upon the course of mental work of the associative value of the separate bits of the materials (discrete stimuli) was investigated by means of three short experiments, in one of which the materials were verbal; in one, pictorial; and in one, numerical.

Verbal Materials—Words in Code

Materials

The materials were made up of three sets of monosyllables written in code, thirty-two in each set. In one set were nonsense syllables, such as *bramm, sab, plor;* in one, connectives and auxiliaries, such as *could, for, here;* and in one nouns and adjectives, such as *slang, car, date.* The lists were matched, word by word, for number of letters.

In order to secure objective evidence as to their differing in associative value, the three sets were presented to several junior college students prior to the experiment. Their judgments were unanimous that the Nonsense Syllables had the fewest associations and the Nouns and Adjectives most. After the experiments, the words were given in scrambled order to the subjects themselves individually in a free association test. The number of associations for each word was recorded, and the totals were averaged for each list. The average associative value per word of Nonsense was 2.25; of Connectives, 3.52; and of Nouns, 6.36.

The associations varied, of course, with the individual, but certain types of response appeared repeatedly. With Nonsense Syllables there were many rhymes, some of them coming after a fairly long pause. For example, *bram* was followed by 'Sam, tam,' and *sab* by 'tab, blab.' With many Connectives a frequently associated word was given; for example, *could* elicited 'go, could come'; and *for*, 'him, for me.' There was more variation with Nouns. *Slang* was followed by 'bad habit, English class'; *car*, by 'gas, oil, tire, wind-shield.' After *kiss* one girl said 'moonlight' quickly, then stopped and blushed.

The code employed was simply the substitution of digits for the letters of the alphabet, taken in order, *1* for *a*, *2* for *b*, and so on. A copy of the code in large type was fastened on the wall just opposite the subject so that he could look at it at will. In his hands was a card with groups of digits typed upon it, each group constituting a word. His task was to substitute letters for the digits, saying the letters aloud and then pronouncing the word.

Procedure

The conditions of this experiment, as of the former, demanded five days of work of each subject. Monday was always devoted to practice which was divided into two parts, a short preliminary period in which directions were given, all the words pronounced, and a few words in code attempted, and a longer period of actual practice upon all the words used in the regular tasks. Each task consisted of four repetitions of the thirty-two words written in a different order each time. The end of each of these four task units was marked on the tape by the assistant who recorded errors. The next day at the beginning of the experimental period, another unit of the same task was given. This unit was called for convenience the *Sequel*.

Since there were three grades of task, there were six permutations of temporal order. Thirty subjects (junior college students) made possible five repetitions of each order.

Results

The work curves for speed and accuracy are given in Figs. 3 and 4.

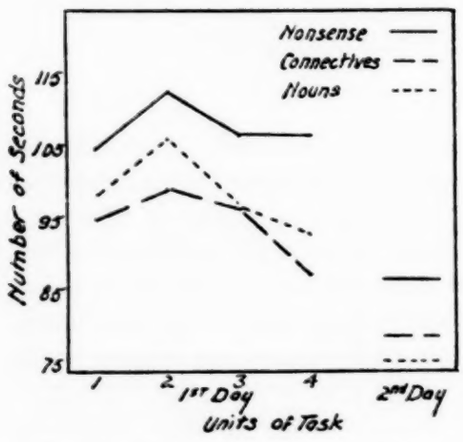

FIG. 3. Time in seconds, verbal materials.

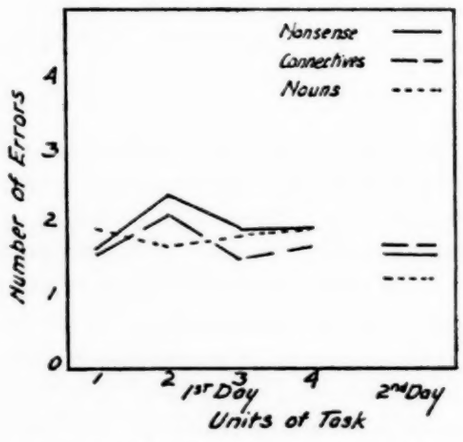

FIG. 4. Number of errors, verbal materials.

With speed as criterion, the decrements for all three materials are highly reliable. They are 20.06 sec. with a standard error of 2.52 for Nonsense, 8.79 ± 1.47 for Connectives, and 17.17 ± 2.91 for Nouns. It is interesting to note that the decrements for the highest and lowest degrees of associative value are very much alike. They show a difference of only 2.89 sec., a figure so low that it is quite unreliable. On the other hand, the decrement for Connectives is 11.27 sec. less than that of Nonsense and 8.38 sec. less than that of Nouns. Both of these differences have high reliability.

In terms of errors, the decrements are .37 for Nonsense, 0 for Connectives, and .74 for Nouns. All three decrements are unreliable, hence their apparently great differences are not significant.

When measured in terms of percentages, the fatigue effect in seconds is 22.9 percent for Nonsense, 11.0 percent for Connectives, and 22.4 percent for Nouns. In errors, it is 23.1 percent for Nonsense, 0 percent for Connectives, and 60.2 percent for Nouns. As has already been pointed out, the error scores are so variable that these figures are more imposing than significant, yet it is interesting that they show the same trends as the time scores.

The levels of work, represented by the average score per task unit, also show these trends, that is, scores which signify low efficiency for Nonsense and Nouns as compared with Connectives. For number of seconds the levels are 108.82 for Nonsense, 95.66 for Connectives, and 99.39 for Nouns. For errors, the levels are 2.00, 1.76, and 1.87, respectively.

Subjects' General Behavior

The behavior of the subjects differed greatly with the three types of material. With Nonsense, there seemed to be little 'getting into' the work. The subjects looked around the room frequently, as if they were distracted by whatever went on. They shook their heads over pronunciations and made many errors, though any possible pronunciation was counted correct. With Connectives the work was steady, and few comments were made. May it be that a connecting

word very often used has the significance of a mild Aufgabe, "Go on"? With the Nouns, two girls and one boy stumbled over *kiss* and *date*.

These facts, together with the type of associations given and the objective results of the experiment, suggest to the writer that with Nonsense the subject had little to hold him to his task and hence was easily distracted, and that with Nouns the discrete bits of the task itself offered distraction through the wealth of association aroused. Both these suggestions need confirmation from experimentation with other types of materials.

Summary

Verbal materials of three grades of associative value were read by means of a code. The materials were Nonsense Syllables, Connectives and Auxiliaries, and Nouns and Adjectives. All three tasks developed reliable decrements with time in seconds, and two tasks, Nonsense and Nouns, showed fairly high decrement with errors.

When the differences in decrement were computed in number of seconds, it appeared that both Nonsense and Nouns differed significantly with Connectives but not with each other. None of the differences were significant with the criterion of accuracy.

Both speed and accuracy reveal three distinct levels of efficiency. Work was least efficient with Nonsense, next with Nouns, and most efficient with Connectives.

The results show that, within the limits of this experiment dealing with verbal materials, a very low degree of associative value and a very high degree of associative value are both more productive of decrement than is an intermediate degree.

Pictorial Materials—Outline Drawings

Materials

The materials consisted of two sets of one hundred outline drawings, twenty-nine different drawings in each set. One series consisted of symbols and abstract forms, such as *bracket*, *cylinder*, and *plus*, and the other of objects and animals, such as *thimble*, *elephant*, and *kite*. The items in the two series were matched as to the number of syllables required in the responses.

As with the verbal materials, a preliminary test was made to discover gross differences in associative value in the two sets. After the experiment was over, free association tests were given to the individual subjects. The symbols showed an average associative value of 2.52, the Objects of 3.26.[2]

The drawings in each series were arranged on a large card, ten rows each way. The subject's task was simply to name the pictures in rapid succession in rows, first across the card, then down, then across, and then down. Each complete naming (one hundred responses) constituted a task unit, and its completion was recorded on the tape by the experimenter.

Procedure

The procedure closely followed that of the preceding experiment except that four work days sufficed instead of five. The first day as before was devoted to practice. With Objects, the chief need of practice was to impress the subjects with the fact that (in order to keep the number of syllables the same in the two tasks) they must say *rabbit* instead of *hare*, and *alligator* instead of *crocodile;* but it was necessary to teach a few subjects the symbols for *bracket*, *oval*, *clubs*, and *spades*.

The subjects were twenty-four junior college subjects.

Results

Figures 5 and 6 give the work curves for the two criteria, speed and accuracy.

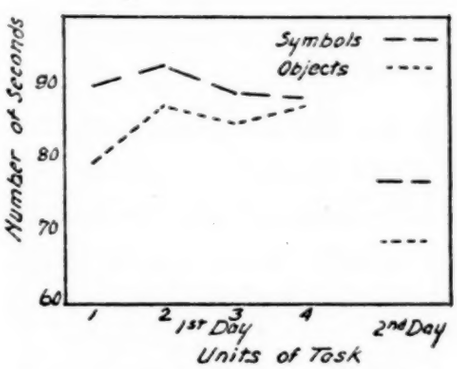

FIG. 5. Time in seconds, pictorial materials.

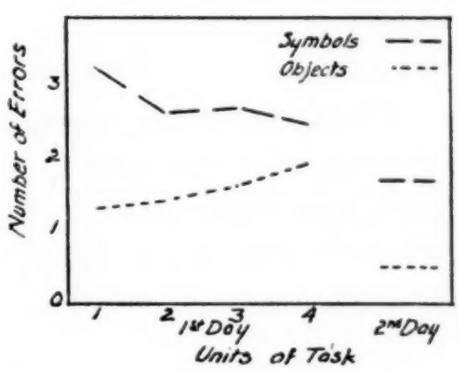

FIG. 6. Number of errors, pictorial materials.

There is a decrement for Symbols of 12.68 ± 2.14, and for Objects, one of 19.46 ± 2.42. Both are highly reliable. The difference between the decrements—6.78 ± 2.87—is also rather large. It yields a critical ratio of 2.4.

With accuracy also, the decrement is greater for the material having the higher associative value. It is .79 ± .47

[2] There was much overlapping. Obviously an improvement upon the present plan would be to ascertain the associative value of a large number of drawings and to group them upon that basis.

for Symbols and 1.17 ± .40 for Objects. The critical ratio of 2.9 for the latter shows that it is reliably high. The difference between the decrements is not significant.

When the decrements are converted into percentages as in the preceding experiments, they are found to be, with seconds a scriterion, 16.7 percent for Symbols and 28.4 percent for Objects, and with errors, 46.2 percent for Symbols and 155.3 percent for Objects. It is interesting that this extraordinarily high error decrement should accrue from the mere naming of perfectly familiar objects and animals, when repetition occurs no oftener with this task than it does with Symbols.

In contrast to all the other experiments in this study, the material with the lower decrement has the work level of lower efficiency. There is an average level of 90.33 sec. for Symbols as compared with 85.16 sec. for Objects. It appears that the relative unfamiliarity of some of the forms and terms made this task actually the more difficult of the two.

Subjects' General Behavior

There were no strong contrasts in the behavior of the subjects with the two types of pictorial material, except for more stumbling and errors in the early part of the task with Symbols and more laughter when errors were made with Objects.

Summary

The task consisted of two sets of outline drawings to be named in rapid succession by the subject. One set consisted of Symbols, one of Objects. When speed was the criterion, both tasks developed reliable decrement, with that for Objects considerably the higher. With accuracy as criterion both tasks again showed decrement, but only Objects to a significant degree. The subjects worked more rapidly, however, with Objects than with Symbols.

The results show that with pictorial material a high degree of associative value tends to produce a high work decrement.

Numerical Material—Dates and Four-place Numbers

Materials

The materials for the third division of the experiment consisted of a list of forty-eight dates important in European and American history and a comparable list of forty-eight four-place numbers without special significance. These lists were prepared with the aid of the instructor in European history, and the dates were those presumably most familiar to the students.

After the experiment was over, lists of the dates and numbers were given to the subjects, who were asked simply to check those that had associations for them, whether or not they were sure they were right. They checked an average of 20.8 dates, and 3.8 numbers.

Each group of digits was written backward. The subject's task was to reverse the order and to read the number properly. Thus the date, *2941*, was to be read *fourteen ninety-two*, and the number, *8631*, *thirteen sixty-eight*. Each task consisted of four complete repetitions of the list of forty-eight numbers, arranged each time in a different order, and on the second day a fifth unit was given, as with the words in the preceding experiment.

Procedure

The procedure closely followed that described for the verbal and pictorial materials. Since only two grades of associative value were used, only four days of work were necessary for each subject. During the practice period all the material was read through once.

The subjects were the twenty junior college sophomores who were recommended as the best history students.

Results

The work curves are given in Figs. 7 and 8.

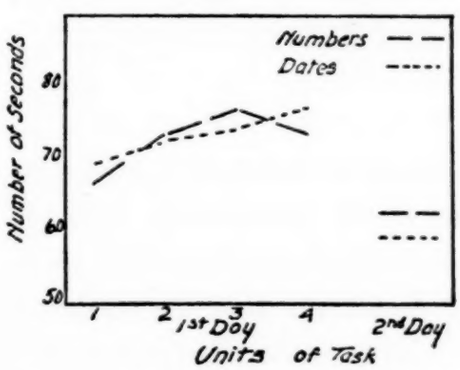

Fig. 7. Time in seconds, numerical materials.

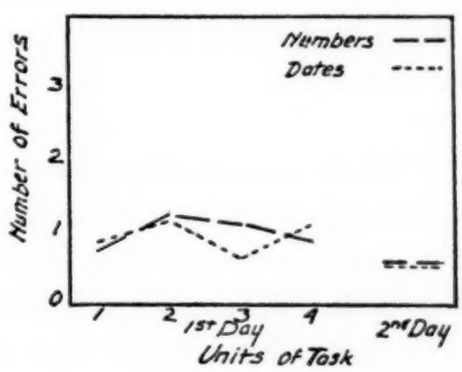

Fig. 8. Number of errors, numerical materials.

In terms of the number of seconds, the decrements are reliable for both degrees of associative value. They are 11.69 ± 1.78 for Numbers and 17.87 ± 1.95 for Dates. The difference between these decrements—6.18 ± 2.32—is also reliable, having a critical ratio of 2.7.

The error scores are more variable than would be expected from the uniform nature of the material. More subjects are obviously needed, but no more good history students were available. The decrements are .30 for Numbers and .60 for Dates. These figures are not high enough to be significant, nor is the difference between them significant.

When the decrements are reduced to percentages of the most efficient work unit, the differences between the tasks are striking. With number of seconds, the decrement for Numbers is 18.8 percent and that for Dates is 30.2 percent. With errors, the decrements are 50 percent and 109 percent respectively.

There is very little difference in the work levels for the two grades of associative value. With time, the averages are 75.13 sec. for Numbers and 75.87 sec. for Dates. With errors, the averages are 1.01 and .96, Numbers having the higher average in this instance.

Subjects' General Behavior

No conspicuous differences appeared in the subjects' attitudes toward these tasks during the work itself, but many comments were made afterward about the dates that were recognized. One boy remarked, "I'd get to thinking about what happened during that year, and I'd forget to go on."

Summary

With the criterion of speed, the task of reading numerical material which was written backward produced reliable decrements for the two degrees of associative value that were studied. Moreover, the difference between the decrements was reliable, Dates, the material with more associations, having the higher decrement.

With errors, the decrements were not reliable, but that for Dates was again the larger.

Dates had a much higher percentage of decrement than Numbers with both speed and accuracy. With speed, work was slightly, but very slightly, more efficient for Numbers than for Dates; and with accuracy, this relation was reversed.

The results show that with discrete numerical material, a high degree of associative value is productive of high decrement.

Conclusions for Experiment II—Associative Value of Discrete Materials

1. When the work materials are discrete and consist of words to be read by means of a code, or of naming drawings in rapid succession, or of reading four-place numbers backward, the decrements developed with the criterion of speed are high enough to be reliable for all degrees of associative value within the limits of this experiment.

2. With the criterion of accuracy, work with these materials is productive of fairly high decrements for all degrees of associative value except for the intermediate degree of verbal material—Connectives—which shows none.

3. Discrete materials of extremely low associative value, exemplified in this experiment only by the Nonsense Syllables, are productive of a greater decrement with both speed and accuracy than are materials with a moderate number of associations.

4. A very low degree of associative value in discrete verbal materials has an adverse effect upon the general efficiency level of mental work.

5. Because of insufficient data, no conclusions can be drawn as to the relative disadvantages of maximum and minimum degrees of associative value. Nonsense showed a higher decrement than Nouns in number of seconds and a lower decrement in number of errors.

6. Discrete materials of extremely high associative value, whether verbal, pictorial, or numerical, are productive of greater decrement in terms of speed and accuracy than are materials of moderate associative value.

7. A high degree of associative value appears to have a somewhat adverse effect upon the general efficiency level of mental work, but the data do not present conclusive evidence.

8. In general, a very low degree and a very high degree of associative value in discrete materials both tend to disrupt mental work and to increase decrement.

No one of these experiments with discrete materials is very convincing in itself. In each one, the work period is short, and the subjects are few. But the fact that all three show the same trends does seem to be significant.

Experiment III

Comprehensibility

The third experiment is concerned with the effect upon the course of mental work of varying degrees of comprehensibility in connected materials.

Materials

Paragraphs were selected that had three points in common and one marked difference aside from subject matter. The subject matter of each is suggested in its title, Dynamics of Mind, Physiological Limit, and Formal Introductions. All described processes, all had similar structures, all had vocabularies of about equal familiarity. They appeared to differ in comprehensibility, the variable to be studied. Prior to the experiment, they were submitted to several students not serving as subjects who arranged them in order of increasing comprehensibility as they are given above.

After the experiment was completed, the subjects themselves were given mimeographed copies of the three paragraphs, following each of which was written: "The paragraph is (not at all, not especially, somewhat, rather, extremely) clear and easy to understand." They were asked to underline the phrase that most nearly expressed their judgment. For scoring purposes the judgments were given the following values: *not at all*—1, *not especially*—2, *somewhat*—3, *rather*—4, *extremely*—5. When computed for the forty-eight subjects the averages were 2.6 for Dynamics of Mind, 4.4 for Physiological Limit, and 4.9 for Formal Introductions. It was considered that these judgments showed that the paragraphed differed sufficiently for the purposes of this study.

The paragraphs were written in code much as were the words in the first experiment dealing with associative value. To render the task not too difficult and to avoid penalizing for the longer words, the initial letter of each word was written as usual, and digits were substituted for the other letters.

In making the paragraphs comparable in form, it was necessary to take a number of factors into consideration: (*a*) the total vocabulary, (*b*) the number of sentences, (*c*) the number of new words in each sentence, and (*d*) the total number of work items per sentence, including the number of words, the number of letters, and the number of substitutions. (*a*) The vocabulary of Dynamics of Mind was reduced to 88 words, that of Physiological Limit to 92, and that of Formal Introductions to 94. (*b*) Each paragraph was rewritten so that it contained eight sentences, each sentence constituting a task unit, with two sentences summarizing the same material for the second day's task. (*c*) The words in the practice period were so chosen that four new words could suffice for each sentence in each task, including those in each Sequel. Effort was made to have these words of somewhat similar difficulty. (*d*) The work items for each sentence included the number of words, the number of letters, and the number of necessary substitutions. It was considered important that the sum of these work items should be

about the same for every sentence of the three tasks. Accordingly minor changes were made in the materials until an approximate uniformity was reached. The sentences in Dynamics of Mind, including the Sequel, were made to average 143.3 items each with an average deviation of 1.2; those in Physiological Limit, 143.3 ± 2.1; and those in Formal Introductions, 143.7 ± 1.2.

Procedure

As in the earlier experiments, the code was plainly printed and placed where the subjects could glance at it at will. Their task was to say each letter of a word aloud, then to pronounce the word, and when all the words of a sentence had thus been given separately, to read the entire sentence aloud. Each of the eight sentences constituted a task unit, and its completion was recorded on the tape by the experimenter's assistant.

Since there were three degrees of comprehensibility, five days of work were demanded of each subject. The practice materials were of especial importance in this case. After some preliminary drill on the code, the subjects read sentences similar in form to those in the regular tasks but not identical with any of them. Care was taken to give the vocabularies equal representation.

The subjects were forty-eight junior college students who had not taken part in the previous experiment in substitution.

Results

Figures 9 and 10 present the detailed results for speed and accuracy respectively.

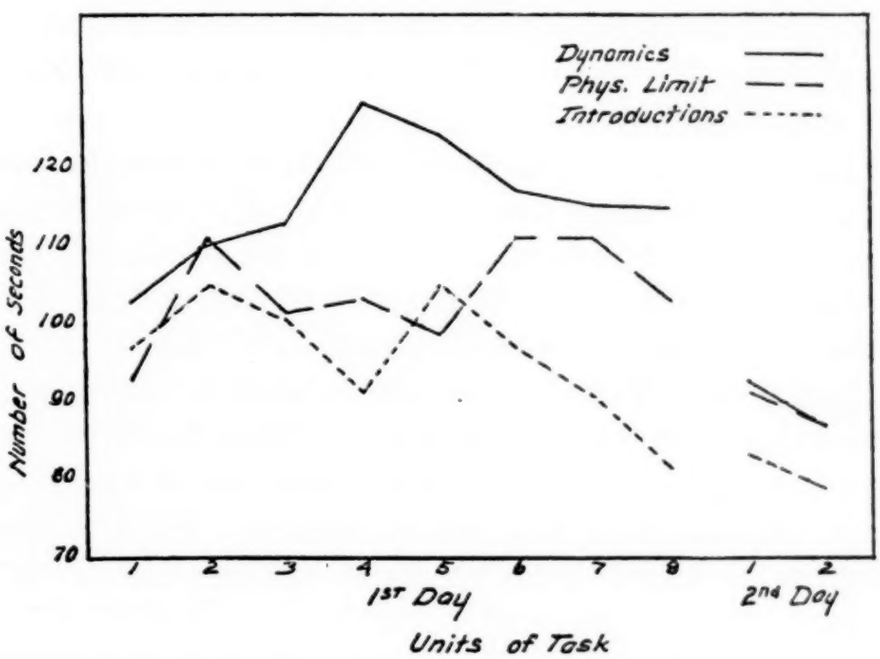

FIG. 9. Average time in seconds per unit of task for three degrees of comprehensibility.

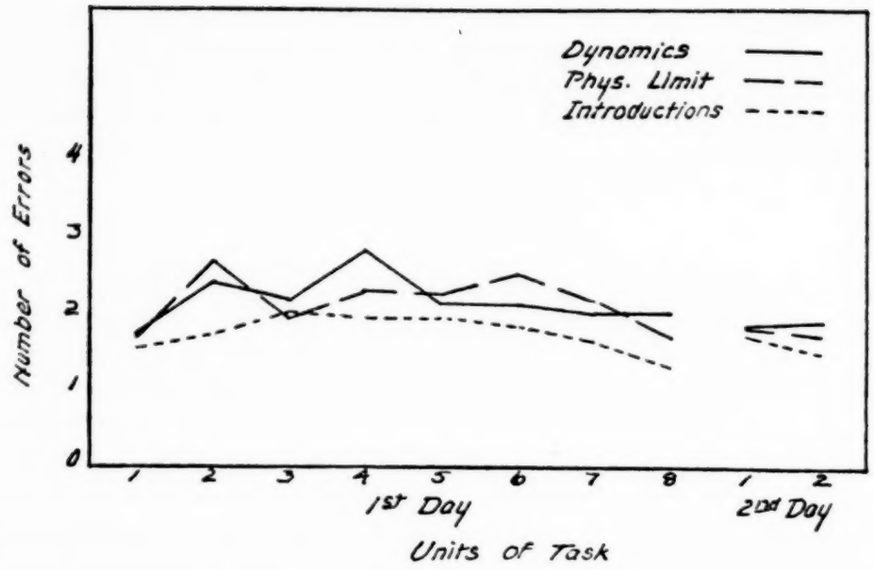

FIG. 10. Average numbers of errors per unit of task for three degrees of comprehensibility.

With the criterion of speed, striking differences appear for the three degrees of comprehensibility. Dynamics of Mind has the large decrement of 22.98 ± 2.76; Physiological Limit, the more moderate one of 11.23 ± 3.28; and Formal Introductions, none at all. As might be expected, the differences between the decrements of Dynamics of Mind and of the other tasks are highly significant. The lowest difference, that for Physiological Limit and Formal Introductions, has a critical ratio of 2.6.

With accuracy, Dynamics of Mind alone shows decrement, and even that not to a reliable degree. The tasks remain in the same order, however, as regards their effects, Formal Introductions losing most by the day's interval. No reliable differences between decrements appear with accuracy.

When time and errors are both reduced to percentages, it is found that with speed Dynamics of Mind has a decrement of 25.5 percent, and Physiological Limit one of 12.4 percent. Formal Introduction has no decrement. With accuracy, Dynamics of Mind alone shows a decrement. It is 10.3 percent.

The levels of efficiency in terms of average number of seconds per unit of task, are more conspicuous in this experi-

ment than in any of the others. Dynamics of Mind required an average of 115.89 sec.; Physiological Limit, 104.43 sec.; and Formal Introductions, only 96.28 sec. The levels for the Sequels were 89.66 sec., 89.09 sec., and 80.73 sec. respectively. With accuracy, the same relative positions are held, but the differences are less. The average for Dynamics of Mind is 2.20; for Physiological Limit, 2.18; and for Formal Introductions, 1.75. The Sequels are 1.89, 1.78, and 1.62 respectively.

Subjects' General Behavior

Of all the tasks in the experiments, Dynamics of Mind seemed to be most disliked. No one broke off in the middle of a day's work, but three subjects refused to come back for the Sequel and had to be dropped from the experiment. Several subjects made apologetic comments on their performance after this task was completed. C. H. said, "I don't know what was the matter with me today. I couldn't keep my mind on it." G. B., who had Dynamics of Mind last of the three tasks, was asked which day's work was hardest for her. She said, "This was hardest, but I guess I didn't keep my mind on it. Seemed like everything bothered me. I kept hearing noises and thinking about that machine."

With Physiological Limit, D. M. said, "I keep guessing, and sometimes I guess wrong." Similar comments were made often with this task and with Formal Introductions.

Only with the latter task, however, was any real interest shown. O. D. remarked when he had finished, "Well, I learned something from that." He and one other boy went back to the laboratory later to make sure they remembered the formulas correctly. This fact suggests that other variables as well as comprehensibility were present in these materials.

The comments of the subjects and the extremely high percentage of decrement found with the least comprehensible materials seem to the writer to indicate that the words of the paragraph, Dynamics of mind, which are quite familiar in other connections but not as they are used here, have

much the effect of discrete stimuli, perhaps even of nonsense syllables. On the other hand the remarkable freedom from decrement found with the more comprehensible materials, together with the comments quoted above and observations of the subjects' behavior, seems to indicate that ready comprehension of connected materials makes possible a forward-looking activity somewhat comparable to that which probably takes place in ordinary reading, though in these paragraphs, of course, the nature of the task precludes a broad eye-voice span.

Conclusions

1. Paragraphs read from a code are productive of a work decrement with both speed and accuracy if their degree of comprehensibility is low.

2. Comprehensibility of connected materials bears an inverse relationship to the decrement developed in the task with both speed and accuracy.

3. Levels of efficiency of performance are proportional to the comprehensibility of the task.

4. Comprehensible connected materials elicit an activity that facilitates mental work and prevents the development of decrement.

General Conclusions and Interpretations

Three variables in work materials in addition to repetition are found to affect the course of mental work. They are connectedness, associative value, and comprehensibility. Their effects are by no means identical.

1. Connectedness of mental work materials bears an inverse relationship to the decrement developed in the work, whether decrement is measured in terms of speed or of accuracy.

2. Very low and very high degrees of associative value in discrete materials are both productive of more decrement than is a moderate degree.

3. Comprehensibility of materials bears an inverse relationship to decrement, work with highly comprehensible materials showing no decrement at all.

4. Discrete materials and connected materials present a contrast in their effect upon the course of mental work: with discrete materials, mental work is performed with low efficiency and the development of considerable decrement, especially if the separate stimuli are very low or very high in associative value; with connected materials, mental work is performed with high efficiency and with little or no decrement, especially if the connected materials have a high degree of comprehensibility.

The earlier experiments with mental work materials, which proved indubitably that repetition is productive of decrement, were developed at a time when the reflex arc concept dominated psychological literature. Some modification of refractory phase seemed to account nicely for results. It is difficult, however, to see how refractory phase (even very liberally interpreted) can be made to explain the decrements developed with the present materials in which the principle of repetition has been rigidly controlled.

In a former paper (**17**), the writer (greatly influenced by the work of Carr and Lashley in their quite different fields) suggested as an alternative hypothesis that mental fatigue is a highly complex phenomenon, which must be explained not merely as resulting from the continuous exercise of a single function but in terms of the total activity of the organism. Total activity must include not only the function involved in performing the assigned task, but all the disrupting and facilitating activities, too, resulting from various central and peripheral stimuli.[3] Maintaining one activity as against the others is a function of the total situation—of the general condition of the individual, of his 'volitional drive,' of the motivation involved, of the nature of the original stimulus, of the strength of the extraneous stimuli, and the like. Mental work, then, in terms of this hypothesis, is the maintenance

[3] Obviously the hypothesis owes much to Carr's discussion of the adaptive act (**4**). "Any adaptive act is always performed in the presence of a wider sensory environment, and all aspects of this sensory situation will affect the organism and release nervous impulses that must find a motor outlet. . . . A stimulus that possesses sufficient motive power may disrupt the act and initiate an adaptive response in reference to itself. Weaker stimuli may function as distractions."

of an activity against encroaching distraction. This maintenance is usually intermittent, and blocking occurs; if prolonged, it becomes a struggle which sooner or later results in feelings of fatigue, and in a work decrement.

Obviously many of the complex factors suggested above are not now accessible to investigation. The present study has been limited to an attempt to investigate one of them—the nature of the stimulus. It is not assumed that the other factors are eliminated but on the contrary that they always interact and that systematic variation of one factor may modify (perhaps exaggerate) the facilitating or disrupting effect of the others.

It appears that not only is homogeneity [4] unfavorable to work efficiency, but so also is a very low degree of associative value in heterogeneous discrete materials. The present hypothesis suggests that these materials are productive of very limited mental activity in themselves, and hence that activity is much subject to the disruptive effect of extraneous stimuli.[5] It appears also that discrete materials of high associative value are likewise unfavorable to work efficiency and are productive of decrement. According to the present hypothesis, the associations themselves arouse activities and halt the progress of work.

On the other hand, connectedness of materials, especially when combined with a high degree of comprehensibility, certainly facilitates work and prevents the development of decrement. An attempt was made to gain some insight into the nature of the facilitating activity. From the fact that comprehensibility seems to be essential, and from clues gained from the subjects' behavior, it is inferred that it is a sort of anticipatory set—not so much a guessing (though guessing sometimes occurs) as an integrative, forward-looking activity that enables the subjects to 'keep their minds on' what they are doing.

Such a concept as anticipatory set is by no means new. In 1892, Sully (**18**) spoke of the value of 'expectant attention'

[4] Homogeneous materials really, of course, constitute the supreme example of discrete materials of low associative value.

[5] This point obviously needs further proof.

in reading. Woodrow (**21**) has shown that reaction time varies with the degree of 'adaptive attention.' Zeigarnik (**11**) working under Lewin's direction, found that unfinished activities were remembered much better than those that had been completed. It may be that this phenomenon, which she and Lewin (**10**) explain through an elaborate analysis of 'tensions,' is related to anticipatory set.

Perhaps the concept of redintegration, as Hollingworth (**9**) interprets it, can be evoked to account for all the variations in effect produced by the stimuli employed in this study. If work materials are discrete and have low redintegrative value, they permit distraction, and a large decrement develops. If work materials are discrete and have high redintegrative value, they disrupt work by virtue of their own redintegrative effects. If work materials are connected and are easily comprehended, they constitute a pattern which the redintegrative mechanism tends to fill out as the work progresses.

This study has attempted the investigation of the effect of three variables upon the mental work decrement. It can end only with the conclusion of Robinson and Bills (**16**), which was quoted above: "Other principles are also surely fundamental in the determination of the decrement, and all of them offer rich territory for exploration."

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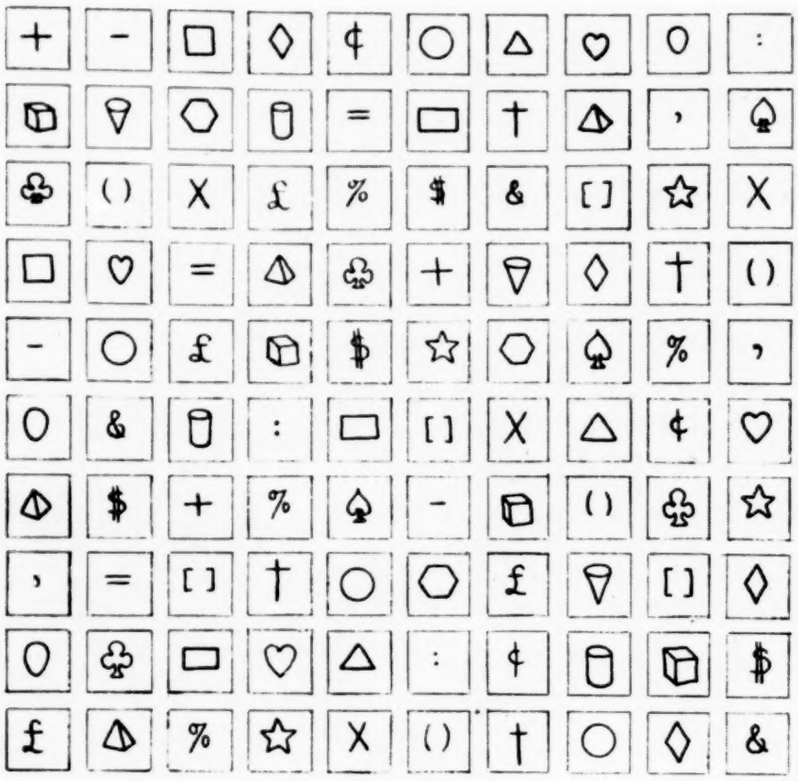
References

1. Arai, T. Mental fatigue. *Teach. Coll. Contrib. Educ.*, No. 54. 1912. Pp. 115.
2. Bills, A. G. *General Experimental Psychology*. New York: Longmans, Green, 1934. Pp. 620.
3. Bills, A. G. and Brown, C., The quantitative set, J. Exper. Psychol., 1929, **12**, 301–323.
4. Carr, H. A. *Psychology: a Study of Mental Activity*. New York: Longmans, Green, 1925. Pp. 432.
5. Cattell, J. M., The time it takes to see and name objects, *Mind*, 1886, **11**, 63–65.
6. Dodge, R., The laws of relative fatigue, *Psychol. Rev.*, 1917, **24**, 89–113.
7. Gates, I. A., An experimental and statistical study of reading and reading tests, *J. Educ. Psychol.*, 1921, **12**, 303–314.
8. Hoche, A. E., Langeweile, *Psychol. Forsch.*, 1923. **3**. 258–271.
9. Hollingworth, H. L. *The Psychology of Thought*. New York: Appleton, 1926. Pp. 329.



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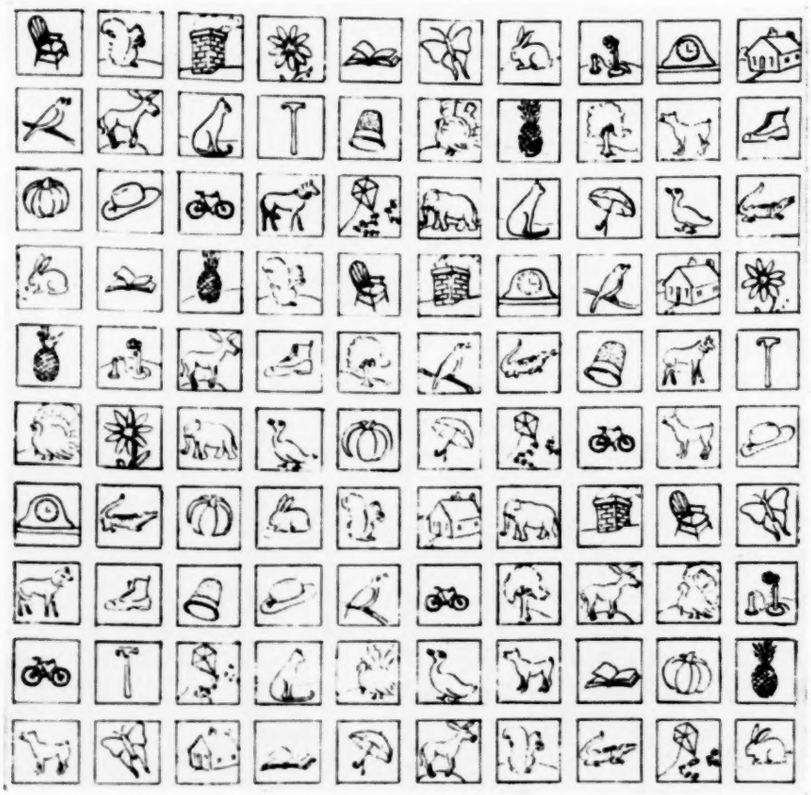

Objects and Animals

10. LEWIN, K. *A Dynamic Theory of Personality.* (Trans. by D. K. Adams and K. E. Zener.) New York: McGraw-Hill, 1935. Pp. 286.
11. LEWIN, K., Untersuchungen zur Handlungs- und Affektpsychologie. III. Zeigarnik, B. Das Behalten von erledigten und unerledigten Handlungen, *Psychol. Forsch.*, 1927, **9**, 1–85.
12. LEWIN, K., Untersuchungen zur Handlungs- und Affektpsychologie. V. Karsten, A. Psychische Sattigung, *Psychol. Forsch.*, 1928, **10**, 142–254.
13. MCGEOCH, J. A., The influence of associative value upon the difficulty of nonsense-syllable lists, *J. Genet. Psychol.*, 1930, **37**, 421–426.
14. PAINTER, W. I., Efficiency in mental multiplication under extreme fatigue, *J. Educ. Psychol.*, 1916, **7**, 25–30.
15. POFFENBERGER, A. T., The effects of continuous mental work, *Amer. J. Psychol.*, 1927, **39**, 283–296.
16. ROBINSON, E. S., AND BILLS, A. G., Two factors in the work decrement, J. EXPER. PSYCHOL., 1926, **9**, 415–443.
17. ROBINSON, M. F., Is the refractory phase theory adequate to explain mental fatigue?, *Psychol. Rev.*, 1931, **38**, 229–241.
18. SULLY, J. *The Human Mind.* Vol. 2. New York: Appleton, 1892. Pp. 393.
19. TELFORD, C. W., The refractory phase of voluntary and associative responses, J. EXPER. PSYCHOL., 1931, **14**, 1–36.
20. THORNDIKE, E. L. *Educational Psychology:* Vol. 3. Mental work and fatigue and individual differences and their causes. New York: Teach. Coll., Columbia Univ., 1923. Pp. 442.
21. WOODROW, H., The measurement of attention, *Psychol. Monog.*, 1914, **17**, No. 76, pp. 158.

THE EFFECTS OF CERTAIN DRUGS AND HORMONES UPON CONDITIONING

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I. Introduction

If a rat is rewarded periodically for pressing a lever, the pressing behavior will come to be fairly constant in rate. This rate may then be used as a base from which to compare deviation produced by variable factors. In this paper the effects of various drugs and hormones in changing the rate of response will be considered.

This technique presents a method of measuring the effects of a reagent upon a habit which has already become firmly imbedded. The learning factor is at a minimum. Therefore, a factor is eliminated which is usually a variable when the behavior of the animal in a maze, problem box, or discrimination apparatus is used for study.

II. Apparatus and Method

Four male albino rats, numbered 1, 2, 3, 4, were used in this experiment. At the start of the experimentation they were about three months old and weighed approximately 200 grams each.

The apparatus used was the Skinner's conditioning boxes (10). Preliminary training consisted of putting the animals in the boxes for one hour on each of several days during which a pellet of food was released automatically at short intervals. This established an association between food and the sound of the food magazine when a pellet is released. During this period the levers were not exposed. After the sound-food association was firmly fixed, the levers were uncovered. The rat in his exploration will accidentally push the lever, immediately releasing the food magazine. Usually after one or two experiences of this kind the rat will again push the lever as soon as he has eaten his pellet. This process will continue until the animal becomes satiated or the experimenter removes him from the box.

The apparatus was then arranged so that food was given only at three minute intervals, the pushes which the rat makes during the intervening interval being unrewarded. Beginning with the third day and every day thereafter food was received every four minutes. In the terminology of the conditioned reflex, therefore, we have a four-minute interval of experimental extinction followed by a reinforcement. Then

there is another period of extinction, a reinforcement, etc., for daily periods of one hour each.

After each daily period the animals were removed to a cabinet in which the temperature was held constant at 25° C. plus or minus 1° C. Here they were fed for one and a half hours if dry food was used, or fifteen minutes if the food was in the form of a mash. The change from dry food to mash was made in the early part of the experiment as it was felt that the food intake might be better controlled. At the close of the feeding interval the amount of food eaten was determined, and the rats remained in the cabinet until the next day's routine began. Water was available at all times except in the conditioning boxes.

This experiment extended over a period of four months. The drugs were given at irregular intervals, care being taken to reëstablish the basic rate of responding before another reagent was used. With the exception of the benzedrine pellets, all drugs were given by subcutaneous injection in a physiological salt solution.

III. Results

In Fig. 1 the results of this experiment are summarized for comparative purposes. It will be necessary to explain the method used in calculation. The records of the number of responses made by a rat in one hour on each of a number of days on which no drugs were given were averaged and used as a base from which to make comparisons. This was done for each animal. The percent of deviation produced by each drug or hormone was then calculated for each rat. The bases of the respective rats were superimposed on the graph and are represented by the solid vertical line to the left labeled *O*. The percent deviation from this base of each rat for each reagent is also plotted and labeled with the rat's number. It was thought desirable to do this as there are large individual differences in reaction to the drugs.

In order, however, to give a more condensed summary of the results, the individual percent deviations were summated algebraically, since some were minus deviations, *i.e.*, the drug decreased the number of responses as compared with the base, and a mean percent deviation was determined. These means are represented by *X* on the graph.

Precisely the same technique of calculation was used for the records of the daily food consumption after all the weights for the mash had been converted to dry weight. The basic food consumption was determined for days on which no drugs were given. The calculations for the food are presented in

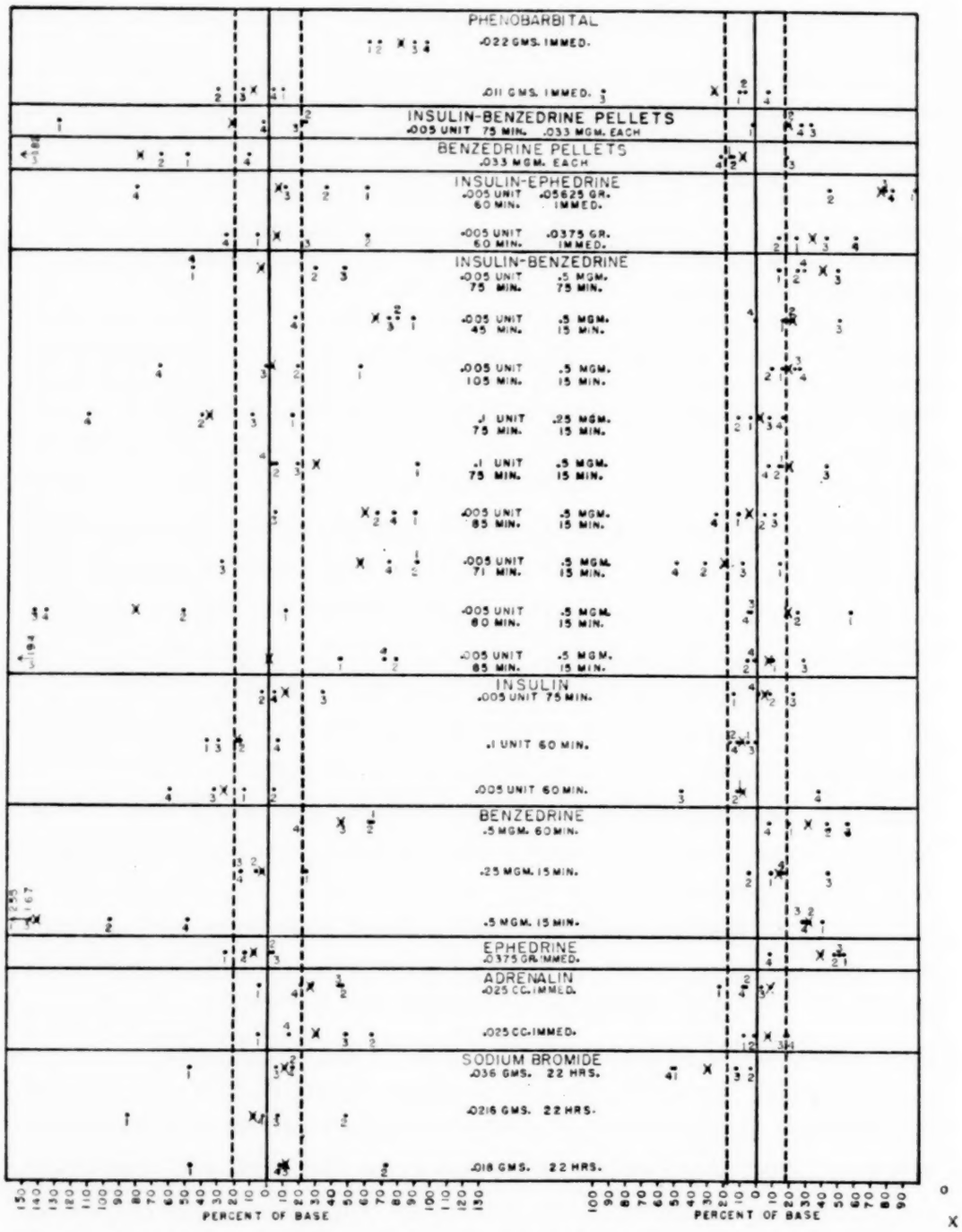
PHENOBARBITAL
.022 GMS. IMMED.
.011 GMS. IMMED.
INSULIN-BENZEDRINE PELLETS
.005 UNIT 75 MIN. .033 MGM. EACH
BENZEDRINE PELLETS
.033 MGM. EACH
INSULIN-EPHEDRINE
.005 UNIT 60 MIN. .05625 GR. IMMED.
.005 UNIT 60 MIN. .0375 GR. IMMED.
INSULIN-BENZEDRINE
.005 UNIT 75 MIN. .5 MGM. 75 MIN.
.005 UNIT 45 MIN. .5 MGM. 15 MIN.
.005 UNIT 105 MIN. .5 MGM. 15 MIN.
.1 UNIT 75 MIN. .25 MGM. 15 MIN.
.1 UNIT 75 MIN. .5 MGM. 15 MIN.
.005 UNIT 85 MIN. .5 MGM. 15 MIN.
.005 UNIT 71 MIN. .5 MGM. 15 MIN.
.005 UNIT 80 MIN. .5 MGM. 15 MIN.
.005 UNIT 85 MIN. .5 MGM. 15 MIN.
INSULIN
.005 UNIT 75 MIN.
.1 UNIT 60 MIN.
.005 UNIT 60 MIN.
BENZEDRINE
.5 MGM. 60 MIN.
.25 MGM. 15 MIN.
.5 MGM. 15 MIN.
EPHEDRINE
.0375 GR. IMMED.
ADRENALIN
.025 CC. IMMED.
.025 CC. IMMED.
SODIUM BROMIDE
.036 GMS. 22 HRS.
.0216 GMS. 22 HRS.
.018 GMS. 22 HRS.
PERCENT OF BASE
PERCENT OF BASE

FIG. 1. Graph of the individual and mean effects of each reagent used upon response output and food consumption.

the graph to the right of the food labels. For both drugs and food all deviations to the right of the zero line are minus deviations, while those to the left are positive, *i.e.*, the drug tended to increase the number of responses or the food consumption, as the case may be.

In order to give the reader some idea of the relative magnitude of the deviations and because the ordinary statistical formulæ for the calculation of significance cannot be applied justifiably to the number of cases here under consideration, the standard deviation of the base for each rat was calculated. The percent of one standard deviation of the base was then determined; these percents were summated. The mean was determined and is represented by the dashed vertical lines on each side of the base line for both the drugs and food respectively. This is helpful chiefly in making a judgment of the significance of the mean deviations rather than those of the individuals.

With this understanding of the derivation of the graph, we may proceed to a consideration of the results. The discussion will follow the order presented on the graph from the bottom up, although that is not necessarily the order in which the drugs were given.

On three successive days .018 gm., .0216 gm., and .036 gm., respectively, of sodium bromide were injected 22 hours before the hour in the apparatus.[1] This drug was used because we are presumably dealing with a phenomenon of conditioning (**10**) (**11**). If such is the case, then Evan's (**3**, p. 359) discussion of the effects of sodium bromide is, to a degree, pertinent. He says that experimental extinction ". . . is now obtained very easily, so that a reflex which takes ten to fifteen repetitions without accompaniment of the unconditioned stimulus to extinguish will now be extinguished after only two or three applications. Owing to their slow rate of excretion, the effect of bromides lasts for two to three days."

Figure 1 shows that from the standpoint of the mean of the group sodium bromide had no appreciable effect. However, it may also be seen that two of the animals were probably affected. Rat number 1 made records consistently higher than his base, while number 2 on two of these days made

[1] This is the only case in which a drug was given on successive days. The reason for so doing was that the first dose seemed to be ineffective from the standpoint of the mean of the group. It was, therefore, decided to give it on successive days since sodium bromide is supposed to have a cumulative effect (**12**, p. 899–906).

fewer responses. In fact, one of the most striking results of the whole experiment was the inconsistency of reaction to a drug from animal to animal. The explanation of this phenomenon is not apparent. The animals were so nearly of the same weight and age that it would seem unreasonable to consider these as effective factors. Also these animals were, relatively speaking, highly inbred, *i.e.*, they are from the laboratory stock of animals which has had no new blood introduced for a period of many years. They should, therefore, be relatively homogeneous genetically. Also from the standpoint of handling in the experimental situation, every effort was made to treat all animals alike.

There is also, to some extent, an inconsistency of reaction in the same individual from time to time. Rat number 2 with three administrations of sodium bromide is a convenient example. One may say that this indicates a lack of proper control of the test situation. There is no way of denying this on a factual basis for any one experimental period. However, the base records show a high degree of consistency in the relative ranking of the rats over a number of test periods.

An alternative is that there are elusive physiological variations in an animal from time to time which are relatively ineffective upon the response output of the organism save in the presence of the drug. Such variations are not easily brought under experimental control.

Although these points are mentioned in reference to the effects of sodium bromide, they, nevertheless, are pertinent, to a greater or lesser degree, in regard to all of the reagents used. To avoid, if possible, monotony and undue length, individual variation will not be stressed particularly in our consideration of the other drugs. However, the reader is invited to inspect Fig. 1 from this point of view.

It is obvious, of course, that under the conditions of this experiment, hunger is the chief motivating influence causing the rat to push the lever. This drive is partially appeased every four minutes by the incentive, food.

It has been the custom ever since the balloon experiment by Carlson (1) to believe that an essential part of the hunger complex is the hunger pangs which in turn seem to depend upon the spasmodic contractions of the stomach. This then is the reason that adrenalin was used as one of the reagents. Among other effects adrenalin inhibits the action of the smooth muscles of the stomach and intestines. If the above assumptions are correct, we would expect that the response rate under the influence of adrenalin would be decreased. This expectation is verified as there is a decrease of 25–30 percent from the base rate. This decrease is accounted for by the fact that the curve of response with adrenalin is negatively accelerated (see Fig. 2).

Ephedrine is a drug which is closely related to adrenalin in chemical structure; its physiological effects are quite similar although there are certain differences. The major differences are the more lasting effect on blood pressure and the stimulation of the central nervous system. Because of the central effect (see the discussion of benzedrine below) one would expect that the rate of response would be higher than that produced by adrenalin. Ephedrine was given only once with no appreciable effect. It would seem, then, that the central effect of ephedrine is sufficient to offset the dimunition of the hunger drive and thus the lowered rate of response caused by adrenalin. If further experimentation verifies the differentiated effect of adrenalin and ephedrine, it may give some interesting clues concerning the fundamental nature of the behavior involved in this conditioning situation.

Benzedrine is a relatively recent drug which has attracted much attention. It is commonly known as 'Pep Pills.' This name arises from the fact that a dose of this drug will produce a mildly exhilarated condition with the disappearance of fatigue and of the desire for sleep. Therapeutically the drug has been used chiefly for the alleviation of the condition known as narcolepsy (13). It does not cure this disorder but simply banishes temporarily the chief symptom, namely, the uncontrollable desire for sleep.

This drug also is similar chemically to adrenalin. In some respects, however, there is a marked difference in the physiological effects of the two drugs. From the behavioral standpoint the most important difference is that of the central effects. This is shown by the effect which benzedrine has upon narcolepsy and also by the fact that benzedrine may be used to destroy phenobarbital anæsthesia (**8**).

Subjectively also the exhilarating effect of benzedrine is much more marked than that of adrenalin. Another subjective effect of particular interest in the present connection is that under the influence of this drug, hunger is greatly reduced or eliminated. If such is the case in the rat, then on the basis of assumptions already made, there should be a marked reduction in the response output unless the reduction is offset by other effects of the drug.

It is also possible that the other factor or factors should not only compensate but be powerful enough to increase the response rate. This is apparently the case. No other reagent which was used had such a strong facilitating effect upon the behavior of the rat in the boxes when given in .5 mgm. dosage 15 minutes before the test. A dose of half this size had no effect which could be determined from the records while a .5 mgm. dose given one hour before the test had an inhibitory effect.

Evidently the effects, either directly or indirectly, of this drug are both inhibitory and facilitory. Do these diametrically opposed factors exist simultaneously with the facilitory effects powerful enough for a time to control the behavior and then weakening, or do the inhibitory factors simply succeed the facilitory? We do not have the evidence with which to choose between these possibilities. It is interesting to note, however, that subjectively in human subjects there is not noticed any depressant phase in connection with the effects of benzedrine.

After having found the large increase in rate of response under the influence of benzedrine, and knowing that the subjective experience is a dimunition of hunger, it was decided that a still higher rate of response might be obtained if in some

way the hunger factor could be maintained. That hunger might be affected in the rat as well as in the human is indicated by an inspection of the food graph. Both times with .5 mgm. dosage there was a decrease in the food consumption.

Insulin is a hormone which in the human subject increases hunger (**3**, p. 289). This is perhaps the natural result of the action of insulin in lowering the blood sugar. It was thought possible, therefore, that if the animals were given a dose of insulin followed by the benzedrine that the former would counteract the hunger-reducing effects of the latter and that thereby a still higher rate of response might be obtained. As a control insulin was given by itself on three occasions. It was given one hour ahead of the test because of its slow speed of reaction. If it increases hunger in the rat, the expectation would be that a higher rate of response would be shown. The records do not show that this expectation is fulfilled in an unambiguous manner. On two occasions there was a slight rise in the mean rate while on the third there was a slight decrease. The food consumption was slightly higher on the days of the higher rates and lower on the third. This would indicate that insulin probably does influence hunger in the rat under certain conditions, but that there is a very delicate balance between the factors producing an increase and those producing a decrease.

The combination of benzedrine and insulin produced results which were totally unexpected. A reference to this part of Fig. 1 shows that in most cases there was a marked decrease in the rate of response. This decrease is especially significant when it is remembered that the same dose of benzedrine given by itself in the same temporal relationship to the test produced a remarkable increase in the rate. The writer is unable to suggest any explanation of the effect produced by combining these two reagents. A number of times the observation was made that the pellets of food which the animal received were left untouched. This never happened when either benzedrine or insulin was given alone. It would indicate, of course, that the hunger drive was completely abolished. On the other hand there was no

consistent or marked variation in the food consumption. Also the animals appeared as lively as usual. There was no indication of a suporific condition.

Perhaps more progress would be made toward an understanding of the behavioral effects of this combination if a number of physiological tests and biochemical analyses were made. These types of investigation, however, lie outside the sphere of the present experiment.

On the other hand, the observed effects may be caused by some direct reaction of the combined drugs upon the central nervous system. If such is the case, the explanation will likely be elusive.

As noted above ephedrine is a drug which is related chemically by benzedrine. Under the conditions of the present experiment, however, ephedrine is not nearly so effective as benzedrine. Nevertheless, it was of interest to try insulin combined with ephedrine. This combination had no observable effect upon the rate of response although it did produce a rather significant decrease in food consumption.

With almost complete certainty one may say that among the probably large number of critical factors in a drug experiment, the dosage is one of the most important. To determine an optimal dosage in the usual way requires a great amount of time and effort. It was thought possible that an approach might be made to this problem in the case of benzedrine by mixing the drug in the food from which the pellets used for reinforcement were made. These pellets contained .033 mgm. of benzedrine each. Therefore, the dose of the drug would be increased each four-minute interval by .033 mgm. This gives a rather fine gradation in the dosage. The expectation was that the rat's record would show a positive acceleration until the optimal dosage was reached.

The records in this particular case were taken over a two-hour period because at the close of the first hour the animals would have received only .528 mgm. of the drug. This dose compares with .5 mgm. which was used when the drug was given subcutaneously. At the close of the two-hour period the rats received approximately twice the dosage used in the remainder of the experiment.

The introduction of a drug orally may result, especially if mixed with food, in quite different consequences from when it is injected subcutaneously. In the case of benzedrine the difference is apparent in the rate of response. While the benzedrine pellets increased the rate over the normal, this effect was more marked with the injection. A comparison of curves *A*, *B*, and *C* in Fig. 2 demonstrates this point.

Curve *B* indicates that the expectation of a positively accelerated curve is not fulfilled. Apparently the animals begin to receive the effect of the drug almost immediately after the first pellet is eaten. This possibly indicates absorption from the mouth. Also there seems to be no cumulative effect of the successive dosages even though the interval between doses is only four minutes.

One trial of a benzedrine pellet-insulin combination was given. This brought the mean level of response down (see Fig. 2) from that of the pellets alone but did not reduce it below the base line.

The remaining drug used in this experiment is phenobarbital. Two dosages were used—.011 gm. and .022 gm. respectively. The smaller dose had no effect upon the rate of response of the animals. The larger dose, which is the minimum anæsthetic dose, however, almost completely abolished the responses after the first few minutes in the apparatus. After the animals had been influenced by the drug to the point that it was evident their response rate would be virtually zero, they were given .5 mgm. of benzedrine. In two rats this temporarily restored the response. Thirty minutes later all animals were again given .5 mgm. of this drug. In the same two rats this was sufficient to increase the rate slightly above what it would have been normally. The other two animals, however, remained at the zero level of response for the remainder of the two-hour test period which was used on this occasion. The records on Fig. 1 are for the first hour only. The animals did not touch their food in the living cages after the large dosage of phenobarbital; therefore, no food records are plotted.

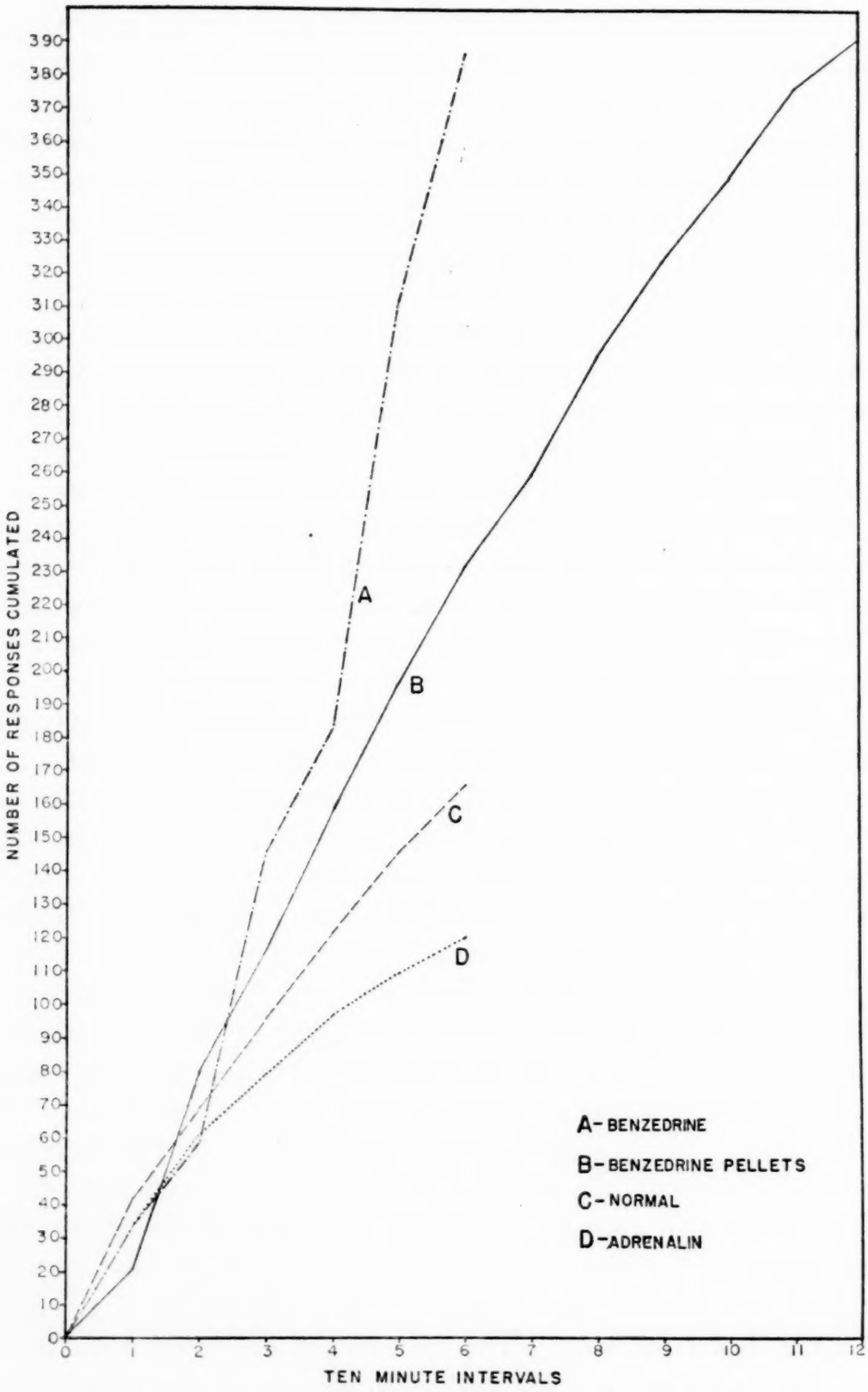

FIG. 2. Curves demonstrating the differential effect of certain drugs upon conditioned activity.

IV. Discussion

The data raise certain questions concerning drive and the interrelation of different drives. Hunger is the chief drive in this experiment, at least on the base days. It has been shown that the rate of response varies with the strength of the hunger drive (**4**). What, then, may be said of the effect upon rate of those drugs (adrenalin, benzedrine, ephedrine) which decrease the hunger drive, *i.e.*, they cause a cessation of the hunger contractions. Of these three drugs only adrenalin gives results which are consonant with the findings of the papers just cited.

In contrast ephedrine does not change the rate, and benzedrine increases it. It has been noted, however, that these two drugs have a central effect which adrenalin does not have. It is obvious that the immediate result of the central stimulation is the release of energy, and in the case of benzedrine, in much greater amount than would be released under a normal degree of hunger. This is not too difficult to understand, but the puzzling question is: What directs the application of his energy to the pushing of the lever? Why does not the energy find expression in random activity? A possible answer is that there is a residuum of hunger and because this, in the past experiences of the rat, has been associated with pushing the lever, it now directs the flood of new energy in that direction. Other experiments (**5**) have shown, however, that the rate of response is increased by benzedrine even though the rat has been completely satiated and with food lying all round it in the apparatus. Under these conditions it is difficult to assume that there is a residuum of hunger.

An alternative answer is that the stimulus complex in the box has become so firmly associated with the behavior of pushing that the rat, so to speak, knows nothing else to do save to push if for any reason it is forced to release energy in the situation. This means that drives, *e.g.*, hunger, thirst, sex, etc., should be interchangeable in the situation and that the effectiveness of each would depend solely upon its energy

releasing ability. Elliott (2) has shown this to be true, within limits, for the maze situation.

Assuming this answer to be correct, a problem immediately arises: To what extent may the stimulus complex be varied without rupturing the association to the point that the energy would be released in a random manner, or, in Gestaltist's terms, how much can the configuration be changed and still have it mean to the rat: Push the Lever?

There is still another possible answer to our question. Pavlov's terminology is applicable. Benzedrine and ephedrine cause a dimunition of internal inhibition. Experimental extinction should be difficult to establish while the animal is under the influence of these drugs, more so with benzedrine than with ephedrine. According to Pavlov (9), caffein has this effect. This has led Hull (6) (7) to certain conclusions and experiments. The effects of caffein have been studied with the technique used in this experiment (5). Benzedrine is much the more powerful, however. Therefore, Hull's hypotheses should be tested with this drug.

The writer while recognizing that each point of view has its advantages, nevertheless is inclined toward the second suggested answer as the one which is the more productive of understanding and experimental test.

V. Summary

Various drugs and hormones were injected subcutaneously into four male albino rats and their effects upon the rate of response in the Skinner conditioning box studied.

Sodium bromide had no appreciable effect upon the mean rate of response. Adrenalin (.025 cc.) decreased the rate by 25–30 percent. Benzedrine in .5 mgm. dosage increased the number of responses by 142 percent. .0375 gr. of ephedrine produced little effect upon the rate of response. .005 unit insulin given one hour before the test increased the rate of response by 26 percent. .1 unit given one hour previous to the test produced an increase of 19 percent.

A combination of .005 unit insulin given one hour and .5 mgm. benzedrine injected 15 minutes before the test

produced a marked decrease in the number of responses in three out of four rats. This effect is variable and difficult to control. .005 unit insulin combined with .05625 gr. ephedrine had little effect upon the response output.

Pellets containing .033 mgm. benzedrine produced an increase in the rate of pushing over the base, but not so high as the rate produced by subcutaneous injection. A combination of .005 unit insulin and benzedrine pellets resulted in a mean increase in rate of 22 percent.

.011 gm. of phenobarbital injected immediately before the test had no effect, while .022 gm. given immediately before the test almost completely eliminated the responses within 10 minutes after the start of the trial. Benzedrine partially restored the response level in two out of four animals.

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References

1. Carlson, A. J., II. The relation between the contrations of the empty stomach and the sensations of hunger, *Amer. J. Physiol.*, 1913, **31**, 175–192.
2. Elliott, M. H., The effect of change of 'drive' on maze performance, *Univ. of Calif. Publ. Psychol.*, 1929, **4**, 185–188.
3. Evans, C. L. *Recent Advances in Physiology.* Phila., Blakiston, 1926.
4. Heron, W. T., and Skinner, B. F., Changes in hunger during starvation, *Psychol. Rec.*, 1937, **1**, 51–60.
5. Heron, W. T., and Skinner, B. F. Effects of caffeine and benzedrine upon conditioning and extinction, *Psychol. Rec.* 1937, **1**, 340–346.
6. Hull, C. L., The conflicting psychologies of learning—A way out, *Psychol. Rev.*, 1935, **42**, 491–516.
7. Hull, C. L., The influence of caffeine and other factors on the phenomena of rote learning, *J. Gen. Psychol.*, 1935, **13**, 249–274.
8. Lagen, J. B., Soley, M. H., and Leake, T. B., Effect of benzedrine sulphate on basal metabolic rate, *Proc. Soc. Exper. Biol. and Med.*, 1936, **35**, 276–278.
9. Pavlov, I. P., *Conditioned Reflexes.* London, Oxford Univ. Press, 1927, 430 p.
10. Skinner, B. F., Conditioning and extinction and their relation to drive, *J. Gen. Psychol.*, 1936, **14**, 296–317.
11. Skinner, B. F., Two types of conditioned reflex and a pseudo type, *J. Gen. Psychol.*, 1935, **12**, 66–77.
12. Sollmann, T. *A Manual of Pharmacology*, 4th ed., W. B. Saunders Co., Phila.
13. Ulrich, H., Trapp, C. E., and Vidgoff, B., Treatment of narcolepsy with benzedrine sulphate, *Ann. Int. Med.*, 1936, **9**, 1213–1221.

STUDIES IN THERMAL SENSITIVITY: 3. ADAPTATION WITH A SERIES OF SMALL ANNULAR STIMULATORS

BY WILLIAM LEROY JENKINS

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In two preceding articles,[1] adaptation phenomena with sets of small circular and rectangular stimulators were reported. Based on the work with circular stimulators, two factors in relation to adaptation time were postulated: an *initial strength* (of sensation) *effect* and a *perimeter effect*, exhibited in different degrees by the four subjects. These postulates were confirmed by the investigation with rectangular stimulators, where the square root of area (related to initial strength of sensation) and the perimeter could be varied independently. In addition, a *configurational effect* upon percentage of response to stimulation and sensation strength was noted.

In a series of annular stimulators, the relation of perimeter to area can be varied still more widely, thus providing an additional check on the original postulates, especially in the case of the subject who showed a pure initial strength effect with circular and rectangular stimulators. A second advantage is that annular stimulators may bring out some evidence as to the *limits* of the perimeter effect. (We cannot hope to keep stretching out a given area thinner and thinner and still expect to find adaptation time purely proportional to perimeter. A breaking point must be reached.) A third advantage is that the annular form should give further evidence regarding the configurational effect noted in the shift from circular to rectangular stimulators.

[1] Jenkins, W. L., Studies in thermal sensitivity: 1. Adaptation with a series of small circular stimulators. J. Exper. Psychol., 1937, **21**, 670–677 2. Adaptation with a series of small rectangular stimulators. Ibid., 1938, **22**, 84–89.

Apparatus and Procedure

In an annular stimulator, it is necessary to have a neutral field on the inside of the annulus as well as around the outside. This requires, in effect, the construction of a stimulator within a stimulator. After repeated attempts to develop a workable annular stimulator with a 10 mm. outside diameter, this was abandoned, because the small dimensions made it impossible to secure a sufficiently rapid water-circulation to maintain the desired temperatures. The stimulators used in this study were based on a 13 mm. outside diameter, with areas equivalent to those of the correspondingly-numbered circular and rectangular stimulators. Their important dimensions are shown in Table 1.

TABLE 1

Dimensions of Annular Stimulators

Stim. No.	Outside Diam.	Width of Annulus	Area sq. mm.	Perimeters		
				Inside	Outside	Total
1	13.0	.030	1.21	40.7	40.8	81.5
2	13.0	.080	3.14	40.4	40.8	81.2
3	13.0	.175	7.16	39.8	40.8	80.6
4	13.0	.309	12.60	39.0	40.8	79.8
5	13.0	.500	19.65	37.8	40.8	78.6
6	13.0	.73	28.3	36.3	40.8	77.1
7	13.0	1.04	38.6	34.4	40.8	75.2
8	13.0	1.34	50.3	32.2	40.8	73.0
9	13.0	1.85	64.5	29.2	40.8	70.0
10	13.0	2.36	78.5	26.0	40.8	66.8

The same four subjects were used as in the earlier series, and the data were taken during the months of February, March, April and May 1937, partially overlapping with the circular and rectangular series. Approximately three sets of readings were taken with each size of stimulator, averaging eight individual readings to a set. During the latter part of April, the apparatus was moved to a basement laboratory where the temperature remained almost invariable, in spite of wide changes outside.

Results

The critical case of Sc-44° (Fig. 1) clearly confirms the earlier postulate that adaptation time in this instance is purely related to the initial strength of sensation and independent of the perimeter. The annular curve interweaves with the other two, and the three of them together show a better relation to square root of area than does any one of them individually. In the right-hand charts, the three well-separated, nearly-horizontal lines are almost at the same levels as those obtained with circular and rectangular stimulators. With this is coupled the same consistency in the

individual readings that was previously noted with the other forms. Strong, then medium, then weak adaptation times occur in order with very little overlapping. (This clear-cut relation, maintained over a six months period, is a tribute to this subject's introspective ability.) In Sm-10° (Fig. 3), the three curves also interweave, still giving evidence of a total absence of perimeter effect.

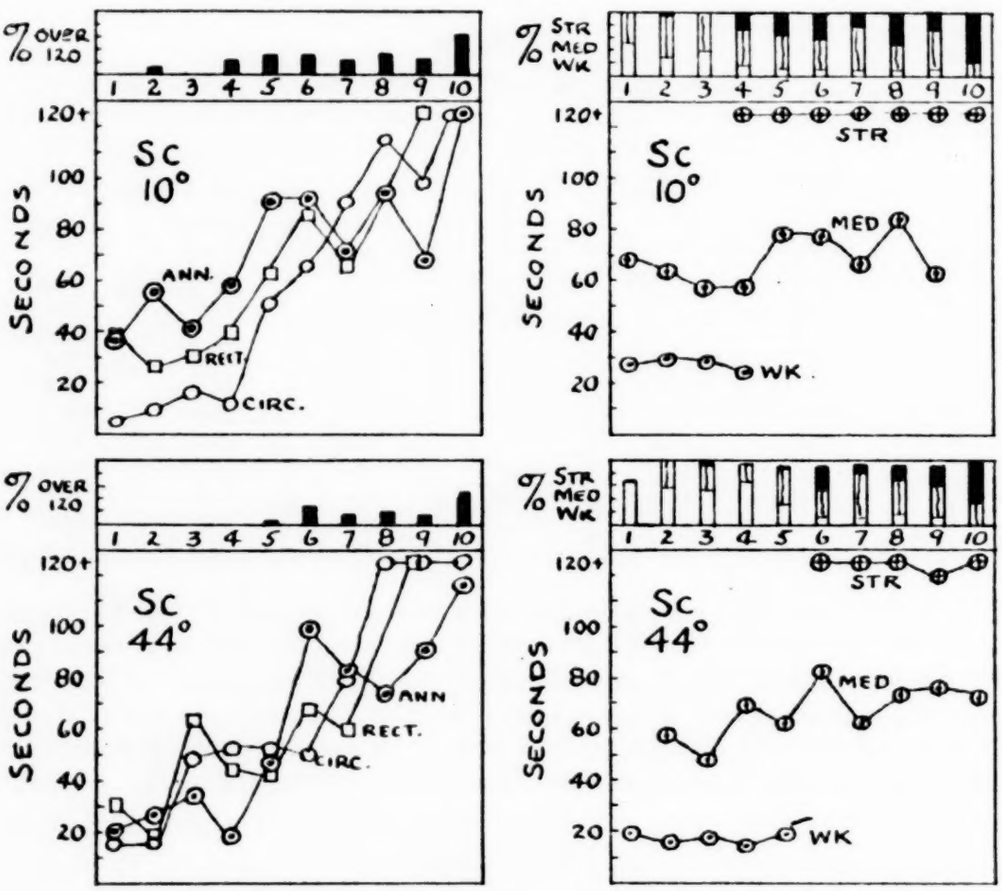

FIG. 1.

Turning now to the charts of Al-10° and De-10° (Fig. 2), which showed practically a pure perimeter effect with circular and rectangular stimulators, we find that the annular values in both cases are above the rectangular. But here, definitely, is evidence of a limit to the perimeter effect. The No. 1 annular stimulator has four times the perimeter of the No. 1 rectangular, but the adaptation time is only slightly increased. Over the curve generally, Al-10° shows a greater increase

than does De-10°. Apparently, then, the limit of the perimeter effect is at a higher level in Al-10°.

From the earlier work we have noted that the perimeter effect in both Al and De is less with 44° than with 10°. This finds definite confirmation in the lower charts of Fig. 2, where the annular curves interweave with those for rectangular stimulators. In Sc-10° (Fig. 1), on the other hand,

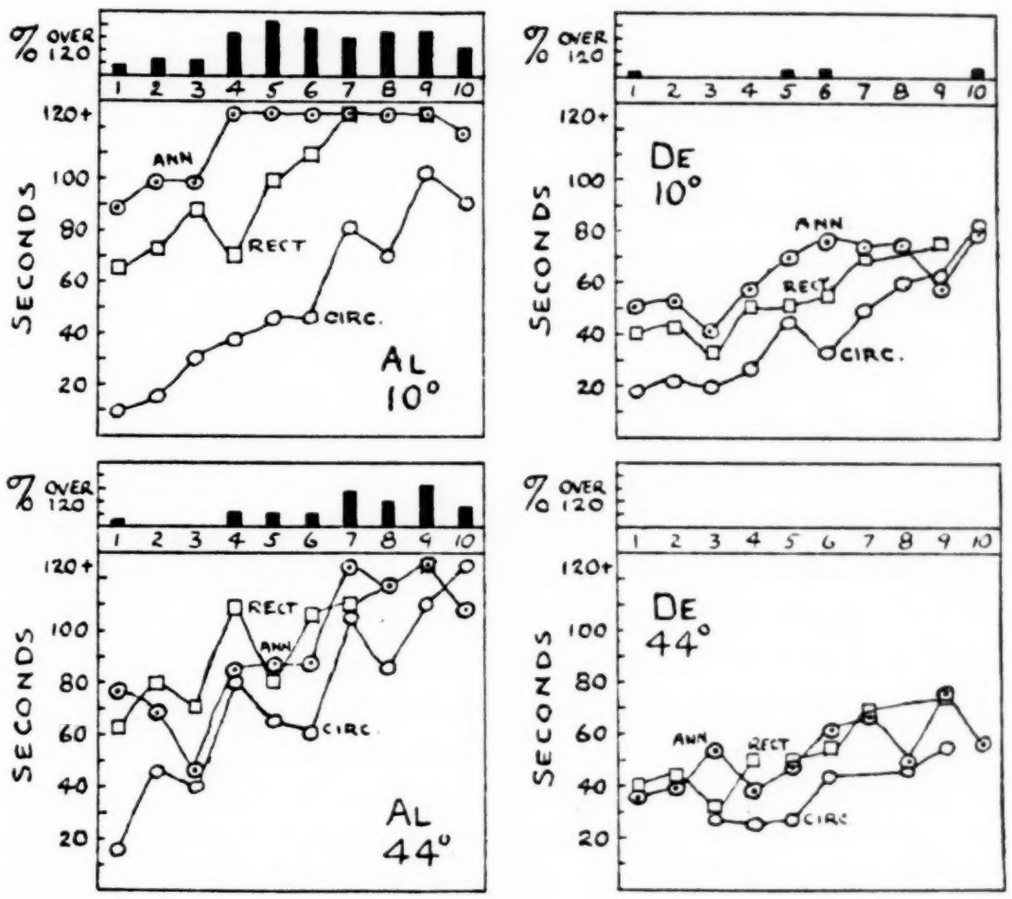

FIG. 2.

the annular values are, on the whole, slightly above the rectangular. We must conclude that the perimeter effect in this last case, although seemingly a minor influence, does not reach its limit at a low level.

Evidence of the *configurational effect* is definitely present. In all subjects but Sm, the percentage of response is higher with annular stimulators than with either rectangular or circular, for both 10° and 44°. The percentage of strong and

medium reports is also higher for 10°, but with 44° this is not true. If anything, the general strength of sensation is slightly less with annular than with rectangular stimulators at 44°.

Reduced Annular Stimulators.—In addition to the regular series of annular stimulators, readings were obtained on three subjects with special stimulators constructed as follows: Two

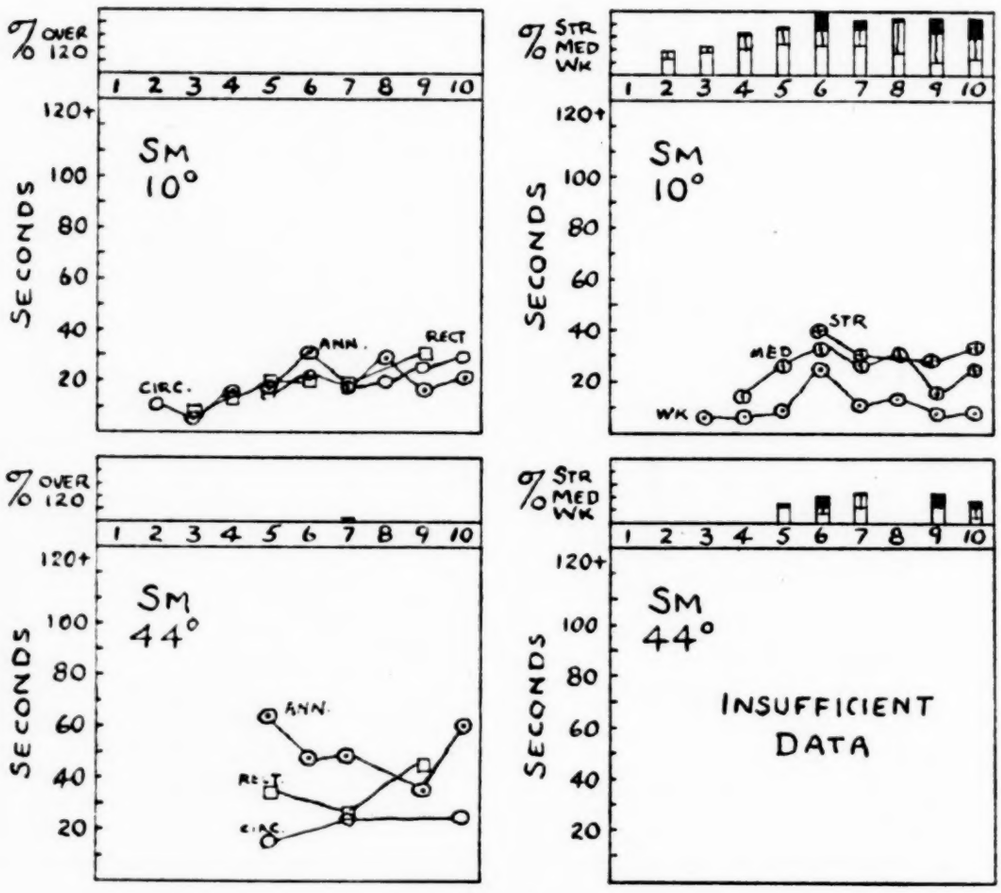

FIG. 3.

No. 5 annular stimulators were made and marked off into 20 equal sectors. In No. 5-A-50 percent, alternate sectors were removed. In No. 5-A-25 percent, three out of every four sectors were removed. In both cases, special inside field discs were constructed to fit. These stimulators provided two reductions of perimeter and area, while still maintaining the general annular form. The dimensions and resulting adaptation times are shown in Table 2.

TABLE 2

RESULTS WITH REDUCED ANNULAR STIMULATORS

Stim. No.	Sq. Root of Area	Perim-eter	Adaptation Time in Seconds					
			Sc.-10°	Sc.-44°	Al.-10°	Al.-44°	De.-10°	De.-44°
5-Ann.....	4.43	78.6	90.9	46.4	120+	86.8	70.0	48.7
5-A-50 percent..	3.14	49.3	62.9	36.9	120+	94.2	84.5	50.8
5-A-25 percent..	2.21	24.7	54.3	23.7	88.0	86.0	50.4	57.0
8-Circ.....	—	25.1	—	—	69.8	86.8	53.8	46.0
5-Rect.....	—	23.9	—	—	98.3	79.2	50.7	55.7

The results provide interesting confirmation of postulates previously made concerning these three subjects. In Sc-44° the reduction in adaptation time is closely proportional to the reduction in the square root of the stimulus area, which is what we should expect with a pure initial strength effect. In Sc-10°, the same trend is noted, but modified upward by perimeter effect. The figures on Al and De must be considered in relation to the limits of the perimeter effect in these subjects. In Al-44° and De-44°, all the perimeters involved are apparently above the upper limit of the perimeter effect. Hence the adaptation times for the standard No. 5 annulus and for the two reduction forms are all the same. In Al-10° and De-10°, the upper limit of the perimeter effect is higher; so in going from No. 5-A-25 percent to No. 5-A-50 percent, the adaptation time lengthens, although it does not double. But with the shift from No. 5-A-50 percent to the standard No. 5-Ann., the upper limit of the perimeter effect seems to have been exceeded and no increase in adaptation time occurs. Notice that the median adaptation times obtained with No. 5-A-25 percent in De and Al check very closely with those of the circular and rectangular stimulators having the same perimeter.

DISCUSSION

If the foregoing results, together with those obtained with circular and rectangular stimulators, can be taken as sufficient evidence of an *initial strength effect* and a *perimeter effect*, some attempt at interpretation is now in order.

Initial strength effect, on the face of it, presents no great difficulties. Other things being equal, we should rather expect to find that the stronger initial sensation lasts longer. Even with subject De-10°, this is true. With any single size and shape of stimulator, strong and medium sensations give longer adaptation times on the average than do weak sensations. (The separation is not too clear, and there are many individual exceptions. Sometimes a weak sensation will actually outlast a strong sensation.) In subject Al-10°, this is also true. But in both these subjects, even with a single size and form of stimulator, something else enters in to obscure the initial strength effect. What this 'something' is we shall have occasion to consider later under the theoretical discussion.

In subject Sc-44°, the initial strength effect appears in such a pure form as to provide an excellent basis for analysis. At least three factors might serve to influence the initial strength of sensation when the stimulating temperature is fixed, as was the case in these experiments: (1) number of receptors stimulated; (2) condition of receptors; (3) central perceptual effects. Each of these merits a separate consideration.

(1) *Number of Receptors Stimulated.*—We have no direct knowledge of the distribution of thermal receptors. (Mapping so-called 'warm spots' is beside the point. We do not know that 'warm spots' actually represent individual receptors. Since the appearance of the map varies with the size of the stimulator used, we may strongly suspect that they do not.) From the percentage of response to stimulation with the smaller stimulators, however, we may make some estimate of receptor distribution. In Sc-44°, the percentage of response is relatively high; hence we shall assume that the distribution of warmth receptors in this subject is relatively dense and rather uniform.

If this is true, the number of receptors stimulated is closely proportional to the area of the stimulator. From the combined curves in Fig. 1, it appears that the length of adaptation time is roughly proportional, not to the area of the stimulator, but to the square root of the area. This

would indicate that the initial strength of sensation is related, not to the number of receptors stimulated, but to the *square root* of the number of receptors stimulated, or to the square root of the number of nerve impulses per second. This situation is reminiscent of the Fechnerian relation between stimulus and sensation.

(2) *Condition of Receptors.*—In a long series, receptor condition should not act as a differential determinant of adaptation time with various sizes of stimulators, but it would account for *variability* in the individual readings for a single run and in the medians from period to period. (Non-uniform distribution of receptors might account for variations in a single series, but not for wide fluctuations in medians from period to period.)

Sc-44°, more than any other subject, showed variability that was nothing short of astounding, especially in the medium range of stimulator sizes. For the same stimulator, the median might be 20 or 30 seconds in one trial, and then go up to 100 or more a week later. Individual readings in a given run covered the same sort of a range. In this case, then, we conclude that there was marked variability in receptor condition from period to period, and in various parts of the arm during the same run.

(3) *Central Perceptual Effects.*—These must be considered doubtful at present. There is plenty of evidence of what we have called the configurational effect—the increase in total percentage of response and in sensation strength when rectangular and annular stimulators were used in place of circular stimulators. However, this may be purely a peripheral phenomenon. Just as a line or a ring will touch more squares on a chess board than will a disc of the same area, so also may a rectangular or annular stimulator reach more receptors than the corresponding circular stimulator. If this is true, our so-called configurational effect becomes merely a matter of more receptors stimulated. If there is actually a central perceptual effect, part of it may be due to thermal contrast, a possibility which offers a chance of independent investigation.

Perimeter effect, so marked in the cases of Al-10° and De-10°, cannot be ascribed to variations in the number or condition of receptors. It might be written off as a purely central perceptual phenomenon, which would place it safely beyond the bounds of experimental attack at present. It is much more fruitful to consider perimeter effect an expression of some features of the peripheral receptor processes. Since nothing at all is known directly as to the nature of the thermal receptor processes, this necessitates making some assumptions regarding them.

A Theory of the Thermal Receptor Processes

In a previous investigation, employing six subjects, the shifts in liminal temperature were studied with a 1¼ mm stimulator, working repeatedly on the same mapped spots.[2] From the depression of liminal temperatures following cold stimulation, their unstable restoration by high temperatures, and the varied shifts in limen which occurred when the temperature of the surrounding field was slowly raised or lowered—a theory of the cold receptor process was developed.

This theory postulates a pseudo-reversible reaction $A \rightleftharpoons B$, with a third substance C which is necessary for the recovery phase. (No assumption is necessary at this time concerning the nature of A, B and C, each of which may be a single substance of a group of several.) The reaction is termed 'pseudo-reversible,' because substance C is not necessarily one of the original constituents. An analogous pseudo-reversible reaction seems to occur in the case of visual purple, with its dark-adapted (A) and light-adapted (B) states. Since the purple state is reconstituted only under special conditions (*i.e.*, in the living eye), it is not unreasonable to assume that some third substance (C) is required to bring about the restoration. A more homely analog is the zeolite water-softener, where a third substance, sodium chloride, must be added to bring about the restoration of the zeolite to its original state.

[2] Jenkins, W. L., Adaptation in isolated cold spots, *Amer. J. Psychol.*, 1937, **49**, 1–22.

According to the theory, stimulation by exposure to low temperatures causes the transformation of A into B, which process initiates nerve impulses. But simultaneously, the reverse action is taking place. With the aid of C, either directly or as a catalyzer, B is being changed back into A. The better the supply of C, the faster will be the rate of reconstitution of A, and the longer the time that the reaction A → B can continue before A is exhausted.

Let us assume: (*a*) that the rate of A → B depends upon the amount of A present at any given moment; (*b*) that the number of nerve impulses per second is a function of the rate of A → B; (*c*) that complete adaptation occurs when the total number of nerve impulses per second falls below a certain necessary minimum value. Then it follows that, in a single receptor, the adaptation time will be a function or either the amount of A present at the start, or the richness of the effective C-supply, or some combination of the two. With a number of receptors stimulated, the same principle applies. The adaptation time will depend upon the amount of A present in the various receptors, or the richness of the effective C-supply in the various receptors, or both.

Let us make two further assumptions: (1) That the amount of A present at the start will typically be correlated with the amount of C. This is not unreasonable, because under normal conditions, A is probably being broken down and reconstituted continually. (2) That even a large amount of A would be exhausted rather quickly, unless there is some C present to bring about reconstitution. In support of this assumption, it may be pointed out that strong initial sensations sometimes die out very quickly, especially in subject Sm. Combining these two assumptions, we arrive at the conclusion that the initial strength of sensation is *not* a true causal factor in determining adaptation time, but is purely epiphenomenal. A certain area gives a strong initial sensation, because a rich C-supply is present and has assured a good amount of A. The same area likewise gives a long adaptation time, because the rich C-supply reconstitutes A rapidly and keeps the reaction going. Thus a good amount of A (strong initial

sensation) may be closely correlated with a long adaptation time, but is nevertheless not a true causal factor. In short, *the richness of the C-supply is virtually the sole determinant of adaptation time.*

If the *same* amount of C determines both the amount of A present (strength of initial sensation) and the length of the adaptation time, there should be a close relation between initial strength of sensation and adaptation time. If a *different* amount of C determines the amount of A present at the start and the length of adaptation time, there might in extreme cases be no relation at all between initial strength of sensation and adaptation time. A number of intermediate stages might also be found.

If the same amount of C determines both initial strength of sensation and the adaptation time, it appears that the effective C-supply must be virtually unchanging during the period of adaptation. For convenience, we may call this a *static* C-supply. With a static C-supply, there should be a close relation between initial strength of sensation and adaptation time. If a different amount of C determines the initial strength of sensation and the adaptation time, the effective C-supply must change during the period of adaptation. This we may call a *mobile* C-supply.

How could the effective C-supply change during the period of adaptation? *The C-supply in a given area might be reinforced by fresh C which diffuses in from the surrounding region.* Logically, this diffusing C would affect primarily the receptors near the border and possibly only a very narrow band of them. If the original C-supply under the stimulator is used up, only this thin rim of receptors around the margin might keep the reaction going and prevent the sensation from dying out entirely. Thus, the ultimate length of adaptation time would be determined solely by the action of this thin line of receptors around the border.

Here, quite obviously, we have an explanation of the *perimeter effect*, since the number of receptors affected by the diffusing C would be proportional to the perimeter. It is now clear why, in Al-10° and De-10°, adaptation time was

purely proportional to perimeter with circular and rectangular stimulators of widely differing areas. It is also clear why this perfect relationship breaks down with the annular stimulators used. There must be a limit to the stretching-out process (the thinness of the line of receptors around the margin), and in the annular forms this limit has been exceeded. The greater the receptor density, the higher will be this limit. The situation is also complicated by the number of receptors in the whole stimulated area. As the total number of receptors increases, their combined action may be sufficient to carry the reaction along for a considerable period of time on the basis of the C-supply present within the area. Thus, with larger areas, perimeter effect should be relatively less important, and the original C-supply relatively more important. We should then expect to find (as we do) more of a relationship between initial strength of sensation and adaptation time in the larger sizes of stimulators. Logically, if the areas of circular stimulators were made successively larger and larger, we should expect to come to a point where perimeter effect disappears entirely. This size was not reached with any of the stimulators used in these studies.

More troublesome is it to explain the apparent *total lack* of perimeter effect in Sc-44°. Here we are on the horns of a dilemma. Two explanations appear possible, but each has a certain air of artificiality about it: (1) In Sc-44°, the Z-supply is static.[3] This would explain both the absence of the perimeter effect and the high correlation between initial strength of sensation and adaptation time. (2) In Sc-44°, the Z-supply is not used up in the process of reconstituting A but merely acts as a catalyzer. Thus, even if the Z-supply was augmented slightly by fresh Z which diffused in from the margin, this would be relatively of little importance because the adaptation time would be determined by the total Z-supply within the area. (It would be possible to assume that C also acts purely as a catalyzer in Al-10° and De-10°

[3] In the absence of contrary evidence, we are assuming that the warm receptor process is analogous to the cold . . . a pseudo-reversible reaction $X \rightleftharpoons Y$, with a third substance Z necessary for the recovery phase.

where strong perimeter effect is shown, if the C-supply under the stimulator is *destroyed* by the stimulating temperature.[4]) Either of these hypotheses concerning Sc-44° will explain the experimental facts. Neither is highly satisfactory, possibly the static Z-supply less so. If our general theory holds, it is clear that the Z-supply in Sc-44° is not at all static in the larger sense. From period to period, the whole general level of adaptation times shifted up and down in a striking fashion. This would be the case if the Z-supply as a whole showed corresponding shifts. If we are actually dealing with a static Z-supply, it must be static only in the narrower sense. On the other hand, the 'catalyzer' explanation leaves us with the necessity of showing why C is destroyed in Al-10° and De-10°, but Z is not destroyed in Sc-44°. In either event, we are dealing with questions beyond the range of our experimental data.

It was noted earlier that in Al-10° and De-10°, the length of adaptation time was not perfectly correlated with the strength of initial sensation, even with the same size and form of stimulator. This appears to be related indirectly to perimeter effect. Since adaptation time in these cases is determined solely by perimeter effect, it must be that the perimeter effect itself varies from spot to spot on the arm. That is, that the amount of C which diffuses in varies in accordance with the supply available in the immediate surrounding region.

Having dealt with the critical cases of Sc-44° (pure initial strength effect) and Al-10° and De-10° (pure perimeter effect) it is probably not worth the space to analyze the intermediate cases in detail. In Sm-10°, the C-supply is so low as to make accurate analysis impossible, although superficially this appears to be a replica of Sc-44° on a much lower

[4] In the earlier experiments with other subjects, there was some evidence which was interpreted to mean that C is destroyed by low temperatures. When the temperature of the field surrounding a 1¼ mm. stimulator was slowly lowered, the liminal temperature was depressed proportionately for a while. However, in each test, before 20° was reached in the surrounding field, the spot suddenly ceased to respond to stimulation, even with the lowest temperatures available. See *op. cit.*, 15–17 and 20 (e).

scale. In Sm-44°, the receptor density is so low that adequate explanation is impossible.

The explanation of perimeter effect in terms of purely peripheral processes opens the way for considerable further research. Even the ambiguous situation in regard to the lack of perimeter effect in Sc-44° suggests the need for possible critical tests. The theory itself should be considered (as it has proved to be) only a fruitful working hypothesis.

Summary

Tests with annular stimulators upon the same four subjects confirm the general postulates based on the earlier studies with circular and rectangular stimulators of equivalent area. A separate initial strength effect and perimeter effect are exhibited in different degrees among the subjects. A configurational effect is again shown, and upper limits of the perimeter effect are indicated. Attempt is made to explain the findings in terms of the thermal receptor processes. A pseudo-reversible reaction $A \rightleftharpoons B$ is assumed for cold, with a third substance C necessary for the recovery phase. (An analogous process $X \rightleftharpoons Y$ and Z is assumed for warmth.) The length of adaptation time is said to be determined in all cases by the richness of the C-or-Z supply, the initial strength of sensation being epiphenomenal and not causal. In cases of pure perimeter effect, adaptation time is finally determined by the thin rim of receptors around the margin of the stimulated area which are supplied with fresh C-or-Z diffusing in from the surrounding region (that within the area being used up or destroyed). Cases of total lack of perimeter effect are ambiguous. It may be that the C-or-Z does not diffuse in from surrounding regions; or that the C-or-Z acts as a catalyzer which is neither used up nor destroyed. In either case, the adaptation time would be determined by the total C-or-Z supply within the area and independent of perimeter. The theory of the receptor processes is presented only as a working hypothesis which has proved fruitful in suggesting research attacks.

(Manuscript received July 13, 1937)

STUDIES IN THERMAL SENSITIVITY: 4. MINOR CONTRIBUTIONS

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In addition to the major findings previously reported with sets of small circular, rectangular and annular stimulators,[1] a number of interesting minor points developed in the course of these investigations. Rather than interrupt the general trend of argument in the earlier articles, these have been reserved for separate presentation.

1. Paradoxical Cold and Warm

It was hoped that, as a by-product of these studies, a sufficient number of cases of paradoxical cold and warm would result to permit a systematic investigation. However, out of a total of some 9,000 stimulations on the four subjects, only 27 instances of paradoxical sensations occurred. Of these, subject Al accounted for 22 (8 paradoxical cold and 14 paradoxical warm) and subject Sc for the balance (1 paradoxical cold and 4 paradoxical warm). No paradoxical sensations at all occurred with either De or Sm.

In the absence of more adequate data, it is possibly worth while to analyze briefly the available cases, as follows:

(*a*) With circular stimulators, 24 paradoxical sensations; with rectangular 3; with annular none.

(*b*) Paradoxical sensation occurred at least once with every size of circular stimulator, including No. 10, although half of the cases involved either No. 2 or No. 3.

(*c*) The median adaptation time for paradoxical cold was 35.8 seconds and for paradoxical warm 11.4 seconds.

[1] W. L. Jenkins, Studies in thermal sensitivity: 1. Adaptation with a series of small circular stimulators. J. Exper. Psychol., 1937, **21**, 670–677, 2. Adaptation with a series of small rectangular stimulators. *Ibid.*, 1938, **22**, 84–89, 3. Adaptation with a series of small annular stimulators. *Ibid.*, 1938, **22**, 164–177.

(*d*) The sensation was invariably reported as 'weak,' but was apparently otherwise indistinguishable from that with adequate stimulation. Later questioning revealed that the subject usually thought that the experimenter had changed the stimulator temperature.

2. Effect of Irradiation

Since most experiments with small metal stimulators [2] have not made use of a surrounding neutral field to reduce irradiation, it was decided to test the effect of omitting this precaution. The opening in a field disc was enlarged to about 20 mm, and comparative sets of readings were taken from different sizes of stimulators, using this 'unrestricted' field disc for one set and the usual close-fitting field disc for the other. Results were so clear with the first dozen sets of readings, that a longer series was judged not to be worth the time consumed. With the unrestricted field, adaptation time was increased by an average of 56 percent with subjects Sc, Al and De. (For Sm, the level of all adaptation times was so low that no clear case could be made out.) There was no evidence of an increase in either the general percentage of response to stimulation nor in the strength of initial sensations. The cause of the increase in adaptation time might be two-fold: (1) irradiation might merely increase the effective size of the stimulated area; (2) but more probably, adaptation time is lengthened because the stimulating temperature is constantly spreading to new and previously unstimulated receptors. We conclude that, in adaptation studies at any rate, it is safer to use a close-fitting neutral field around the stimulator to minimize irradiation.

3. Reaction Time to Thermal Stimulation

In the work with circular, rectangular and annular stimulators, reaction time measurements were taken as a

[2] For examples, see: M. B. Drury and K. M. Dallenbach, The response of cold spots under successive stimulation, *Amer. J. Psychol.*, 1931, **43**, 490–496; R. H. Earhart and K. M. Dallenbach, The response of warm spots under successive stimulation, *ibid.*, 1933, **45**, 722–729; S. Aronoff and K. M. Dallenbach, Adaptation of warm spots under continuous and intermittent stimulation, *ibid.*, 1936, **48**, 485–490; H. A. Levine and K. M. Dallenbach, Adaptation of cold spots under continuous and intermittent stimulation, *idem*, 490–497.

matter of routine, except during the first month of the circular series. A synchronous motor chronoscope was used with a typical break-and-break circuit, the break for the start being supplied by a Bakelite block in the cam which moved the stimulator down against the skin. The subject was instructed to react as soon as he felt definite thermal sensation; otherwise, to withhold his reaction. Subjects Sc, Al and Sm usually experienced a tactual sensation from the contact of the stimulator with the skin. Subject De, however, never reported 'touch' at all, even with the largest stimulators! (Such low tactual sensitivity in this subject would justify further investigation if time permitted.)

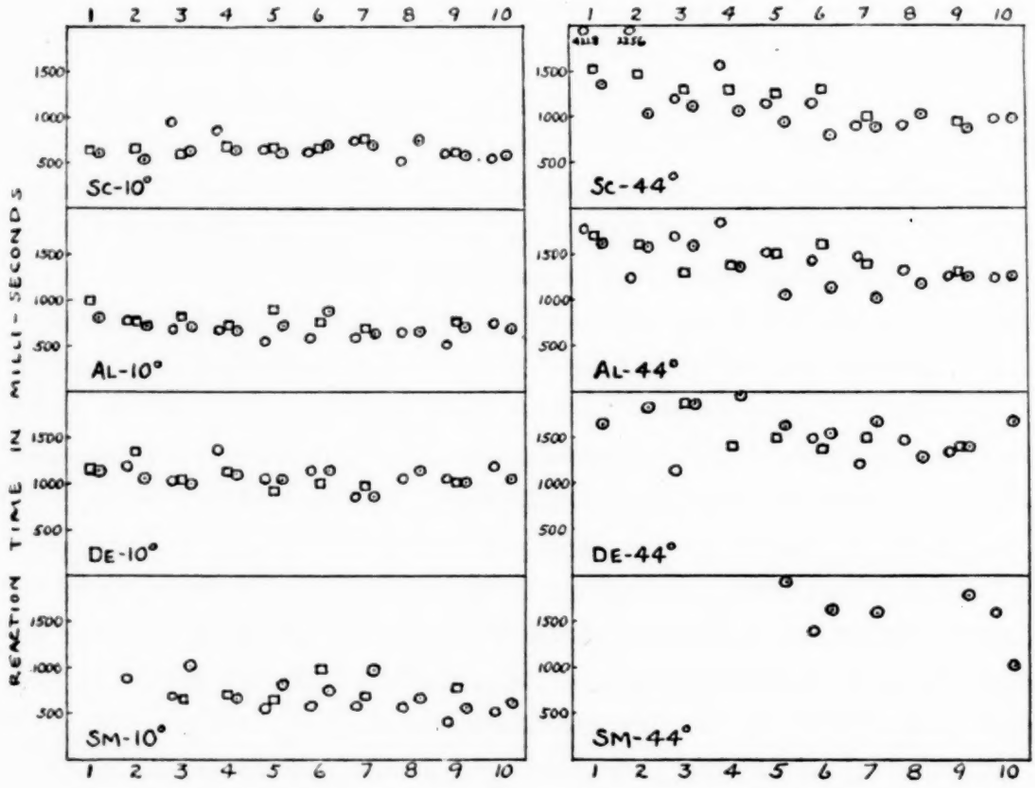

FIG. 1.

Figure 1 shows median reaction times, plotted against stimulator number, which is always proportional to the square root of the area regardless of the shape of the stimulator. The chief points may be summarized very briefly as follows:

(*a*) Effect of stimulator size: With 10° stimulation, there is very little evidence of any effect, the values being almost

identical throughout. With 44° stimulation, size does seem to be an influence, reaction times being definitely longer with the smaller stimulators.

(*b*) Effect of stimulator form: No clear-cut evidence, except for Sc-44° where the rectangular and annular forms gave definitely shorter reaction times in the two smallest sizes.

(*c*) Comparison of cold and warm: In these four subjects, reaction time is definitely longer with 44° than with 10°, by 50 percent to 100 percent. There is also a marked difference in the subjective experience. Cold seems to snap on instantly at its full intensity. Warm starts asymptotically and builds up to a peak. Part of the lengthening of reaction with warm may be due to the difficulty of reporting the *beginning* of an experience of that nature.

4. Variations over a Six Months Period

No systematic study seems to have been made of the variations in thermal sensitivity over a long period of time. By combining the results from circular, rectangular and annular stimulators, we have a more or less continuous record on these four subjects from December 1936 to May 1937, inclusive. The data were taken under what might euphemistically be called 'natural conditions.' In the absence of an air-conditioned research room, temperature and relative humidity could not be fully regulated. The use of students as subjects precluded adequate control of such factors as physiological condition, previous exposure, etc. Whatever the disadvantages of this situation from the standpoint of the major research, it does result in a record of fluctuations in sensitivity such as might occur in everyday life.

Unfortunately for purposes of comparison, a number of sizes and three different forms of stimulators were used. The results, therefore, can be compared only if we state each value in relation to some norm. As a norm for each of the 28 stimulators, the only feasible figure seemed to be the median of all the adaptation times secured with that stimulator, which is certainly a 'norm' only in a very special sense. The 'comparative figure' for each individual group of readings

was then found by stating the median as a percentage greater or less than the arbitrary 'norm.'

The necessary crudeness of this procedure must be kept in mind in examining the charts of Fig. 2, in which the 'comparative figures' are plotted against time. Briefly, the results may be summarized:

(*a*) *Variability.*—This is large in all cases and possibly greatest with Sc-44°. In general, it is greater for 44° than for 10°. Also, the spread is much wider in some experimental periods than in others. Sometimes the 'comparative figures' for a given day are closely grouped; occasionally they are almost identical (which provides some left-handed justification for our arbitrary 'norm').

(*b*) *General Trends.*—Fluctuations in the general level from period to period are plain, and there is some indication of more general movements involving several weeks. As a prime example, the general level of Al-44° rose markedly from December to February (so much so that the 'comparative figures' had to be adjusted to bring the circular data in line with those for rectangular and annular stimulators). A much slighter, but similar effect is to be observed with De-44° and Sc-44°. Outside of this, evidence of seasonal trends, as such, is lacking. The curve of Sm-10° bows downward in the middle and is high in December and May, but it is difficult to interpret this as a seasonal trend.

The causes of variability within a given period and shifts in general level from period to period might well be made the subject of further systematic research. In general, we might expect to find three types of causes: (1) environmental (temperature, humidity, etc.); (2) psychological (especially the criteria for deciding when a sensation has died out completely); (3) physiological (including possibly variations in the richness and mobility of the C-or-Z supply [3]).

[3] This is the hypothetical factor which primarily determines the length of adaptation time, according to the theory of the receptor processes proposed in the third article of this series. See: W. L. Jenkins, Studies in thermal sensitivity: 3. Adaptation with a series of small annular stimulators. J. Exper. Psychol., 1938, **22**, 164–177.

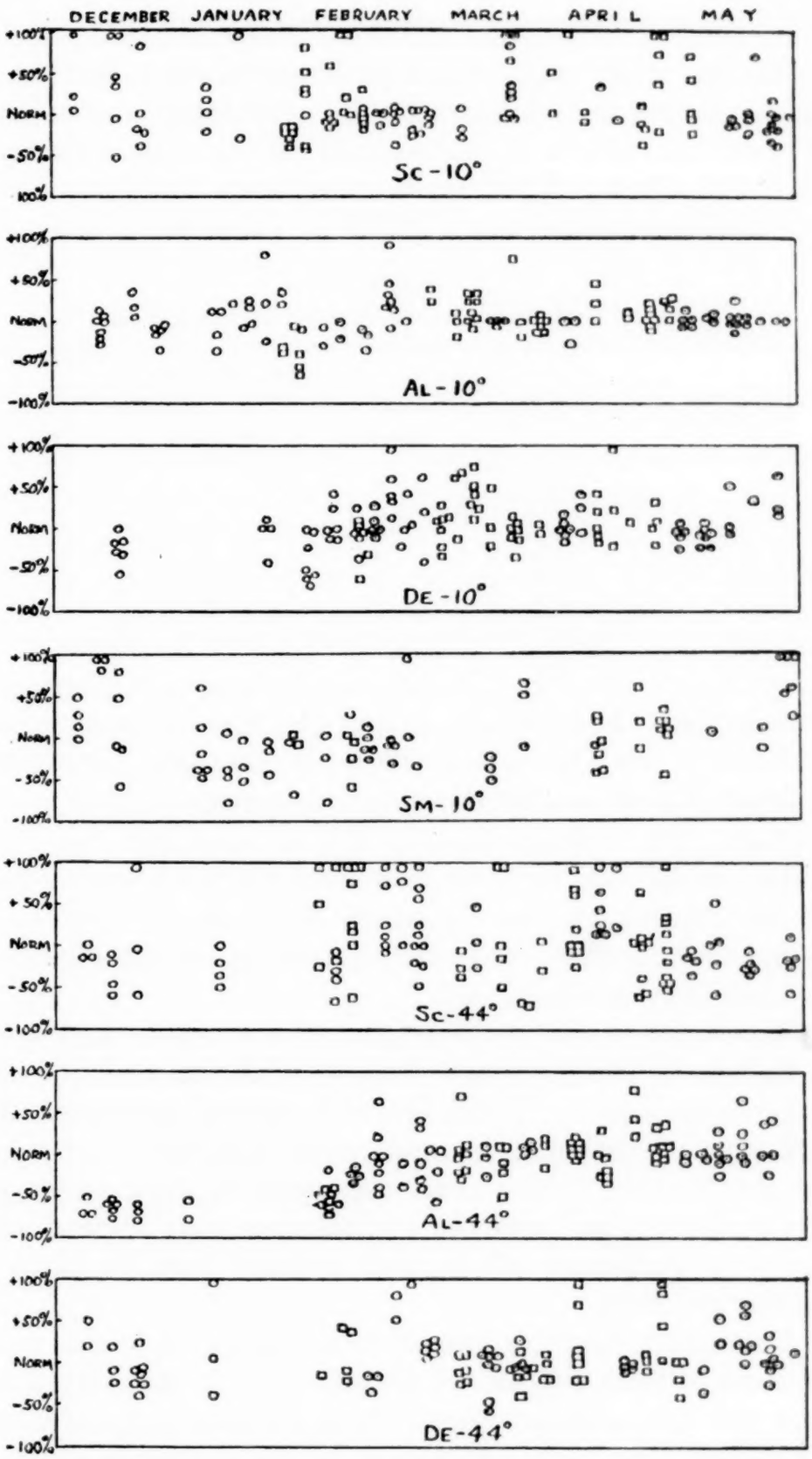
DECEMBER
JANUARY
FEBRUARY
MARCH
APRIL
MAY
+100%
+50%
NORM
-50%
-100%
Sc-10°
AL-10°
DE-10°
SM-10°
Sc-44°
AL-44°
DE-44°

FIG. 2.

5. Effect of Stimulator-Orientation

In the study with rectangular stimulators, half of the measurements were made with the stimulator longitudinally (L) and half with it at right-angles (R) to a line joining the wrist and elbow. The results of separate analysis of these two orientations are summarized in Table 1. (In Sm, the

TABLE 1
Effect of Orientation—Rectangular Stimulators

Subject and Temperature	Increase L over R	Number of Times		
		L Greater	R Greater	Equal
Sc-10°	17%	5	2	1
Sc-44°	7%	3	5	0
Al-10°	12%	4	2	2
Al-44°	18%	5	2	1
De-10°	13%	5	3	0
De-44°	11%	3	3	0

values are all so low and irregular as to make such comparison inadvisable.) For subjects Al and De, there appears definite evidence of an orientation effect—in that the longitudinal position (L) gives slightly longer adaptation times on the average. For subject Sc-10°, this is also the case. For subject Sc-44°, the increase in general median is contradicted by the number of cases in which R is greater than L. It seems, then, doubtful that the orientation of the stimulator is of any influence in Sc-44°.

This leads to the possibility that this *orientation effect* may be tied up in some intimate fashion with the *perimeter effect* earlier mentioned. Al and De both showed perimeter effect (influence of perimeter as such upon adaptation time), as did also Sc-10°. These cases also seem to show orientation effect. But Sc-44° was the critical case that showed absolutely no perimeter effect, and appears here to show no orientation effect.

Perimeter effect has already been explained as due to the diffusion of fresh supplies of C-or-Z into the stimulated area from the surrounding region. It is at least suggestive that adaptation time is slightly longer (where perimeter effect has

been established) when the stimulator is placed parallel to the main direction of blood-circulation. However, the differences are too slight to provide more than a hint—to be checked, if possible, by direct research.

Summary

This report covers a number of minor points developed in studies with circular, rectangular and annular stimulators.

1. Paradoxical cold and warm were experienced only 27 times out of 9,000 stimulations, and by only two of the four subjects.

2. When the stimulator is not surrounded by a neutral field, irradiation results in lengthening adaptation time by about 50 percent.

3. Reaction time is definitely longer for warm than for cold by 50 percent to 100 percent in all four subjects. Warm reaction time is affected somewhat by stimulator size, but cold reaction time is scarcely affected. The form of the stimulator (circular, rectangular or annular) seems to have little influence in any case.

4. Thermal sensitivity (as measured by length of adaptation time) varied markedly over a six months period, both in general level and extent of variability, but there was no clear-cut evidence of true seasonal trends.

5. With rectangular stimulators, the longitudinal position (in relation to the wrist-elbow line) developed slightly longer adaptation times than the right-angle position in the cases where perimeter effect had been established. The one subject who did not show perimeter effect does not show this orientation effect. A possible tie-up of orientation effect with blood-circulation is suggested.

(Manuscript received July 13, 1937)

EVIDENCE FOR BOTH STIMULUS-SUBSTITUTION AND ORIGINAL ANTICIPATORY RESPONSES IN THE CONDITIONING OF DOGS *

BY W. N. KELLOGG

Is there in conditioning, the actual production of qualitatively different behavior? Or is there only the learning to do an old thing under new circumstances? Shall we think of the experimenter who observes the development of a new conditioned response, as not unlike the mariner who witnesses a volcanic island appear 'from nowhere' before his eyes? Or shall we say that the *CR* is a case of sensory transfer in which an unconditioned response becomes connected to a new or different stimulus? Is the response emergent and original? Is it radically different from the unconditioned response? Or is it old in the organism's experience, and simply newly elicited?

This question has been widely discussed in experimental and theoretical literature, and remains at present a point of difference between two psychological schools of conditioning. On the theoretical side the cause of a new response has been championed by Tolman (**16**, **17**) and more recently by Hilgard (**4**, **5**). On the experimental side a clear-cut distinction between conditioned and unconditioned responses has been specifically noted in the measurement of respiration by Wever (**20**), Horton (**8**), and others, in the flexion response by Liddell (**13**), in the eyelid response by Hilgard and his collaborators (**3**, **6**, **7**), in the knee-jerk by Wendt (**19**), and in other responses by Hudgins (**9**), Warner (**18**), and Schlosberg (**15**).

The doctrine of stimulus-substitution has a much longer history, and may be regarded as starting with Pavlov (**14**). It has lately been given a new lease on life by Guthrie (**2**), who has taken the position that the response which occurs

* The research work, upon which this publication from the Indiana Conditioning Laboratory is based, was supported by a grant from the University.

when the conditioned stimulus is introduced is the response which is conditioned. The experimental background of the sensory-transfer or substitute-stimulus hypothesis is also much the greater of the two, since the majority of the Russian experiments have usually been classified here. It is probably safe to say that practically all conditioning experiments made prior to a decade ago were generally assumed to be examples of stimulus-substitution.

In view of the existence of these two divergent views, our own observations upon the nature of the flexion response in the conditioning of dogs may prove of interest. For, during the course of some recent experiments, we seem to have obtained evidence in support of *both* of these hypotheses.

Experimental Procedure

The dogs were conditioned by the buzz-shock technique described in greater detail elsewhere (11, 12). The buzz was sounded for 2 sec. and the shock for approximately the last .2 sec. of the 2-sec. interval. During the training the animals were strapped loosely in a solid wooden stock, with the forepaws fastened to hinged and balanced levers, of very light weight. These permitted a vertical movement of each front foot of about 4 inches. All other parts of the body had considerable latitude of movement, excepting the neck which was held by a wooden collar. A tambour system attached to the hinged levers permitted accurate recording of the flexion responses of the forepaws. If an animal raised its foot a distance of approximately 2½ inches just before the electrical stimulation, this activated a mercury switch that permitted him to escape or avoid the shock. Experimenter and apparatus were separated from the subjects by a one-way screen.

Discussion of Typical Results

The reactions compared here are confined entirely to conditioned and unconditioned *flexion* responses. Struggling and other preliminary or habituating activity, which may either be regarded as an aspect of generalized conditioning, or as part of the adjustment to the whole situation, have not been considered relevant. Our observations refer, in other words, to the development of the localized or adaptive *CR*, in its relation to the unconditioned foot-withdrawal to shock.[1]

[1] Culler and his associates (1) have pointed out that localized conditioning often develops as a refinement of the generalized struggle response. It has been our observation that such a description will not apply to all animals. Some dogs, for example, do not struggle at all. Others, we have found, are never able to eliminate the struggle response, in spite of the fact that a conditioned flexion reaction is readily developed.

A careful study of the characteristics of the response, as shown in its recorded picture, has demonstrated that two kinds of reactions are common. Sample reproductions from original records, which show the two varieties of conditioned flexion movement, are presented in Figs. 1, 2, 3, and 4. Figures 1 and 2 illustrate the development of the "conditioned reflex" or substitute-stimulus type of reaction. Figures 3 and 4 show the new and original variety of *CR*. In all four figures, *R* is the response line, tracing the activity of the right forepaw, to which the shock was delivered in the examples shown. *B*, *S*, and *T* are the buzz, shock, and time lines respectively. Time units are in .2 sec.

In Fig. 1 are typical reflex responses to the shock alone, which occur during early training. These we have found to be *the same for all animals*, regardless of the kind of *CR* which is later developed. They are very rapid, of maximal height (about 4 inches), and similar in contour or configuration. In Fig. 2, the transition from unconditioned response to the 'conditioned reflex' or substitute-stimulus type of response is apparent. The reaction farthest to the left in Fig. 2 is a characteristic response to the shock alone. The reaction in the middle (Fig. 2) is a *CR*, since the animal has advanced it into the buzz period and has 'escaped' the shock. The reaction to the right in Fig. 2 is a *CR* in which the shock has been omitted by the experimenter. All three of these responses (together with those of Fig. 1) are strikingly similar with respect to their height, duration, speed of lift, and contour. They have the appearance of abrupt, reflex activities. The only major difference between the conditioned and the unconditioned responses of this sort is one of latency —the *CR*s occurring sooner after the beginning of the buzz stimulus. In consideration of this temporal difference, it may be argued that an absolute *identity* between conditioned and unconditioned responses cannot be demonstrated. Yet

In cases like the latter, the generalized reactions seem to go on quite independently of local responses to specific stimuli. One way of stopping the struggling of certain dogs has been, surprising as it may seem, to sound the buzz. Since struggling and flexion conditioning are mutually inhibitory, the animals usually become quiet at once and make the flexion response.

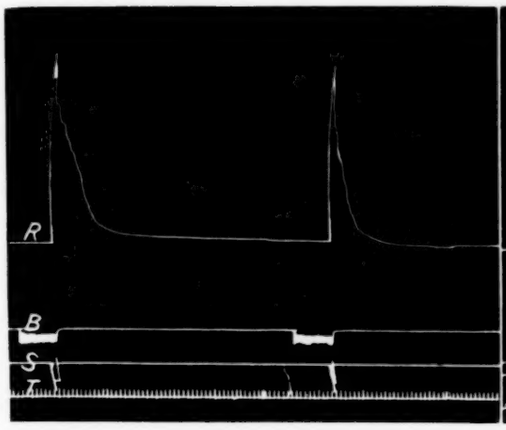

FIG. 1. Unconditioned flexion responses.

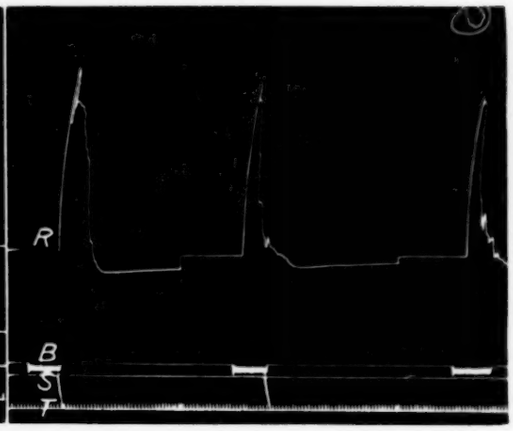

FIG. 2. The transition to conditioned 'reflex.'

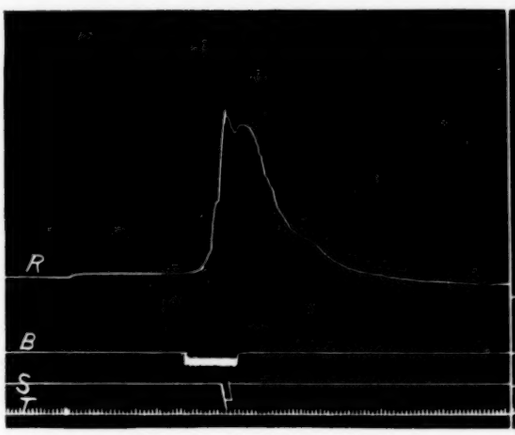

FIG. 3. 'Deliberate' *CR* combined with an unconditioned response.

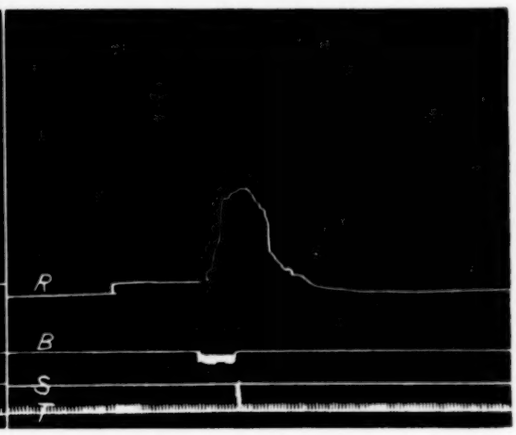

FIG. 4. 'Deliberate' conditioned escape.

FIGS. 1, 2, 3, and 4. These figures show two types of conditioned flexion response of the right forepaw, in dogs. In all four figures, *R* is the response line; *B*, *S* and *T* are the buzz, shock and time lines, respectively. In Fig. 1, typical unconditioned lifts to the shock are shown. Figure 2 illustrates the substitute-stimulus or conditioned 'reflex' type of *CR*, while Figs. 3 and 4 show the deliberate or gradual variety.

The response to the left in Fig. 2 is to the shock alone, but the remaining two responses are conditioned lifts to the buzz. In the middle reaction (Fig. 2) the animal has escaped the shock by lifting his foot before the shock arrives, and in the right-hand *CR*, the shock has been omitted by *E*. All of these reactions are of the same general form as the unconditioned responses in Fig. 1.

In Fig. 3, a gradual or deliberate type of *CR*—obviously different from the lifts in Figs. 1 and 2—is begun by the animal in response to the buzz, only to be interrupted before it is complete by the reflex response to the shock. Figure 4 shows a successful escape of the deliberate variety.

there is good evidence, in these cases, for saying that the *conditioned flexion movement*, itself, is essentially the same as the *unconditioned flexion movement*. Instead of appearing when the shock occurs it is now attached to that part of the buzz which comes before the shock.

In Fig. 3, however, a new and deliberate type of response is seen which, from the record, would appear to be more like a 'voluntary' reaction. The animal begins his foot-lift shortly after the buzz has started and accelerates it as the buzz proceeds. By the time the shock arrives, the foot has been raised almost (but not quite) enough to escape the shock. In the particular example shown in Fig. 3, the result is a reflex (unconditioned) withdrawal, superimposed upon the still uncompleted conditioned reaction. The dual response illustrated offers a nice contrast between the first part of a deliberate *CR* and later parts of a sudden unconditioned withdrawal. It should be noted that the dog was in the process of making a conditioned response for nearly two full seconds before the shock was administered. In the substitute-stimulus type of reaction, on the other hand, complete lifts in nearly all cases take something less than .2 sec.

In Fig. 4 a successful 'escape' of the deliberate variety is registered. Here the subject—again slowly and easily—has raised his forepaw high enough to cut out the shock completely. The reaction differs not only in latency from the unconditioned responses in Fig. 1, *but in height, configuration, speed of movement, and duration as well.*

The particular kind of *CR* which ultimately develops would seem to depend upon the subject conditioned. It need not be *exactly* like one or the other of the reactions which have been presented. Sometimes there appears a kind of pumping movement which resembles a series of conditioned 'reflexes' that continue throughout the buzz period. This we would take to be a special case of stimulus-substitution in which the response repeats itself in rapid succession. Combination or intermediate types of response are also possible, but they too seem, in the main, to be characteristic of the animal.

In most instances it is not difficult to fit the response given under one or the other of the ideal types described.

To the best of our observations, the sort of flexion conditioned reaction made by a particular animal tends to remain constant after long training. It is so much a function of the animal that the same general kind of movement occurs in both right or left forepaws. Observations on bilateral transfer of conditioning have established this point (**12**). In only one dog have we noted a tendency to shift from the 'deliberate' to the 'reflex' *CR*, and in no instance to date have we had a case of shifting in the opposite direction. Generally speaking, the whole question of the type of *CR* which is developed reduces itself to a subheading under the broader topic of individual differences. Is it not possible then that both the doctrine of stimulus-substitution and that of original anticipatory conditioned responses are correct? And that each may find evidence to support it, depending on the kinds of experimental subjects which are used? This thesis has been developed at greater length and from a slightly different point of view elsewhere (**10**).

Summary

Evidence has been presented to show that, in the case of the flexion response of the forepaw in dogs, it is possible to distinguish two varieties of conditioned reaction. Intermediate or combination responses have also been found. One of the extreme varieties resembles (except in latency) the unconditioned flexion response which is made to a shock stimulus. The other variety differs in latency, amplitude, speed of lift, duration, and configuration from the unconditioned response. These two kinds of responses seem to offer experimental justification for both the substitute-stimulus theory of conditioning and the view which holds that the conditioned response is a new and original reaction.

(Manuscript received September 10, 1937)

References

1. Culler, E., Finch, G., Girden, E., and Brogden, W., Measurements of acuity by the conditioned response technique, *J. Gen. Psychol.*, 1935, **12**, 223–227.
2. Guthrie, E. R., *The Psychology of Learning.* New York, Harpers, 1935.
3. Hilgard, E. R., Conditioned eyelid reactions to a light stimulus based on the reflex wink to sound, *Psychol. Monog.*, 1931, **41**, No. 184, 50 p.
4. Hilgard, E. R., The nature of the conditioned response: I. The case for and against stimulus-substitution, *Psychol. Rev.*, 1936, **43**, 366–385.
5. Hilgard, E. R., The nature of the conditioned response: II. Alternatives to stimulus-substitution, *Psychol. Rev.*, 1936, **43**, 547–564.
6. Hilgard, E. R., and Campbell, A. A., The course of acquisition and retention of conditioned eyelid responses in man, J. Exper. Psychol., 1936, **19**, 227–247.
7. Hilgard, E. R., and Marquis, D. G., Acquisition, extinction and retention of conditioned lid responses to light in dogs, *J. Comp. Psychol.*, 1935, **19**, 29–58.
8. Horton, G. P., A quantitative study of hearing in the guinea pig (*Cavia cobaya*), *J. Comp. Psychol.*, 1933, **15**, 59–73.
9. Hudgins, C. V., Conditioning and voluntary control of the pupillary light reflex, *J. Gen. Psychol.*, 1933, **8**, 3–51.
10. Kellogg, W. N., An eclectic view of some theories of learning, *Psychol. Rev.* (in press).
11. Kellogg, W. N., and Walker, E. L., 'Ambiguous conditioning,' a phenomenon of bilateral transfer, *J. Comp. Psychol.* (in press).
12. Kellogg, W. N., and Walker, E. L., An analysis of the bilateral transfer of conditioning in dogs, in terms of the frequency, amplitude, and latency of the responses, *J. Gen. Psychol.* (in press).
13. Liddell, H. S., The conditioned reflex. Pp. 247–296 in *Comparative Psychology* (edited by F. A. Moss). New York, Prentice-Hall, 1934.
14. Pavlov, I. P., *Conditioned Reflexes.* (Trans. by G. V. Anrep.) London, Oxford Univ. Press, 1927.
15. Schlosberg, H., Conditioned responses in the white rat, *J. Genet. Psychol.*, 1934, **45**, 303–335.
16. Tolman, E. C., *Purposive Behavior in Animals and Men.* New York, Century, 1932.
17. Tolman, E. C., Theories of learning. Pp. 367–408 in *Comparative Psychology* (edited by F. A. Moss). New York, Prentice-Hall, 1934.
18. Warner, L. H., An experimental search for the 'conditioned response,' *J. Genet. Psychol.*, 1932, **41**, 91–115.
19. Wendt, G. R., An analytic study of the conditioned knee-jerk, *Arch. Psychol.*, 1930, **123**, 97 p.
20. Wever, E. G., The upper limit of hearing in the cat, *J. Comp. Psychol.*, 1930, **10**, 221–234.

THE INHERITANCE OF BRAIN POTENTIAL PATTERNS *

BY ABRAHAM BER GOTTLOBER

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While a great deal of work has been done to determine the nature and meaning of brain potentials in individuals, comparatively little experimentation has been directed toward the problem of determining what similarities, if any, exist in the brain potential patterns of members of one family who apparently have other characteristics and traits in common. Davis [1] has found that in the case of identical twins, the brain potential patterns are practically identical. No work has as yet been published on the relationships between patterns of other members of the family group.

To establish the facts of this problem was felt to be a significant and necessary step toward a fuller understanding of the phenomenon of the Berger rhythm.

Subjects

Fifteen families cooperated in this study. Each consisted of father, mother and two or more children over fourteen years of age. The fourteen year age limit was set as it has been demonstrated that in children the rhythmic alpha waves first appeared at about three months of age and at a frequency of three to four per second. The frequency was found to increase with age until the adult average was reached at eight to ten years of age. Between the ages of ten and twelve the frequency increases slightly but returns to the adult average at twelve years of age.[2] A margin of two years was given to be certain the adult rhythm would be obtained.

Apparatus and Procedure

The recording of the potentials was made by means of standard amplifiers and a Westinghouse oscillograph.

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* A condensation of a part of a doctor's dissertation done under the direction of Professor Lee Edward Travis.

[1] H. Davis and P. A. Davis, Action potentials of the brain in normal persons and in normal states, *Arch. Neurol. and Psychiat.*, 1936, **36**, 1214–1224.

[2] D. B. Lindsley, Brain potentials in children and adults, *Science*, 1936, **84**, 354.

The experimental conditions from subject to subject were kept as constant as was possible. The observer had the subject recline on a cot in a dark room. He was instructed to keep his eyes closed and his mind as free from thought as was possible.

A unipolar lead was used, the electrode being placed on the left occipital area. The ground electrode was placed on the lobe of the left ear. An average of about thirty seconds of film was recorded from each subject. This was considered sufficient since in two previous studies [3, 4] the same amount of film served to establish the individuality of the electroencephalogram and its constancy in the same person from day to day.

Data

The data were analyzed as follows. Records of first seven and then eight complete families were pasted on a wall. (Lack of space prohibited exposition of the records of all fifteen families at one time.) Four of the workers in the laboratory who had had considerable experience in recording and evaluating brain potentials judged the records. The reliability of these judges was established to be high in the study on individuality of the electroencephalogram.

The first procedure was to inform each judge (in this case only three of the judges participated) that the records of seven (and then eight) families were being shown. He was instructed to place each record in one of seven (and then eight) groups so that those records in a group would look more like each other than any record in any of the other groups.

The second procedure was to point out to each judge the records of a father and a mother and all the children of each family. The sex of the children was not indicated. The judge was then instructed to match the record of each child with that of his parents and to indicate which of the parents' patterns the child's pattern resembled more closely.

The third procedure was to instruct each judge to reinspect the records, this time classifying each child's record as: (1) resembling one parent's record or the other, (2) both, (3) neither. To be classified under (1), the child's record, in the judge's opinion, would have to show practically no resemblance by the criteria used (*i.e.*, frequency, amplitude,

[3] L. E. Travis and A. B. Gottlober, Do brain waves have individuality, *Science*, 1936, **84**, 532–533.

[4] L. E. Travis and A. B. Gottlober, How consistent are an individual's brain potentials from day to day, *Science*, 1937, **85**, 223–224.

and form of waves as well as characteristics of beta waves, shape of the alpha waves and the relationship between negative and positive deflections) to the record of the other parent. To be classified under (2), the child's record would have to show approximately equal resemblance to the records of both parents. To be classified under (3), the child's record would have to show for the most part more dissimilarities than similarities to both of the parents' records. (This last classification does not conflict with or contradict the second procedure of classifying a child's record with that parent's record which it resembles more than the other.)

This concluded the subjective analysis which, while evaluating the patterns both quantitatively and qualitatively, emphasized the qualitative factors listed above.

Following this treatment, objective, quantitative analyses of the records were made. These consisted of marking off all the waves of a given record which would unequivocally fall into the classification of the alpha type of rhythm (waves having an average frequency of ten per second). The total number of such waves was counted. A time line having a frequency of twenty-five cycles per second plus the use of a constant speed motor for rolling the film during exposure assured an exact measurement of the duration of each wave. The duration of the total record in seconds was determined and the percentage of time occupied by alpha waves was calculated. Finally, the average frequency of the alpha waves was obtained. This was an additional check for the purpose of determining whether the waves selected as being of the alpha type actually had an average frequency of eight to twelve cycles per second. (The length of the record in seconds was considered to consist of those portions in which alpha or beta waves were present. Those portions during which blocking of the amplifiers occurred due to movement, etc., of the subject were not considered as part of the total duration of the record.)

Having calculated the percentage of time occupied by alpha waves in each subject's record, it was then classified as

belonging to one of four groups, *i.e.*, dominant, subdominant, mixed or rare alpha, in accordance with Davis' classification.

Records in which the alpha waves are present from zero to twenty-five percent of the time were called rare alpha type, from twenty-five to fifty percent mixed, from fifty to seventy-five percent subdominant, and from seventy-five to one hundred percent dominant.

Using percentage of time the alpha waves were present as the only criterion for judgment, a comparison of parents' and children's records was again made. In this instance, a child having either a rare alpha or mixed alpha rhythm was considered as falling into the same classification as a parent having either a rare alpha or mixed alpha rhythm; the same of course was true of parents and children having the subdominant or dominant alpha rhythm.

The percentage of children falling into the same classification as only one or the other parent was determined. Following this the percentage of children falling into the same classifications as either and both parents was determined. For example, if the number of sons falling into the same classifications as their mothers was being determined, a son in this comparison would be recorded as falling into the same classification as his mother whether or not he also fell into the same classification as his father.

Results

In reviewing the results of this study, the following facts were observed. Individuals cannot be identified with any members of their family on the basis of similarity of their electroencephalograms. The three judges' groupings indicated that an individual's brain-potential pattern may resemble other individuals' patterns as often as those of members of his own immediate family. The fact that each individual has a pattern peculiar to himself holds true in families as well as in the general population.

The second procedure, in which the percentage of children resembling only one parent or the other was determined, would appear to indicate that there is a tendency for daugh-

ters' records to resemble those of their fathers' more than those of their mothers'; and for sons' records to resemble those of their mothers' more than those of their fathers'; but it must be remembered that the results were forced here, the records of the children being classified with either the father or the mother. None of these differences, however, is statistically significant (Tables I, II).

TABLE I

PERCENTAGE OF SONS AND/OR DAUGHTERS RESEMBLING FATHERS OR MOTHERS (AVERAGE OF JUDGES' RATINGS, WITHOUT CHOICE OF 'NEITHER' OR 'BOTH')

Relationship	Percentage of Former Resembling Latter	N
Son Like Mother	49.99	11.25
Son Like Father	43.47	10.25
Daughter Like Mother	37.50	9.37
Daughter Like Father	60.00	15.00

TABLE II

SIGNIFICANCE OF DIFFERENCE BETWEEN PERCENTAGE OF SONS AND DAUGHTERS RESEMBLING ONE OR THE OTHER PARENT

Relationships Compared (Greater Percent Given First)	Differences in Percent of Resemblance	σ Diff.	$\frac{D}{\sigma \text{ Diff.}}$	Chances
Daughter Like Father vs Son Like Father	16.53	.150	1.1	86
Daughter Like Father vs Daughter Like Mother	22.50	.154	1.46	93
Son Like Mother vs Daughter Like Mother	12.49	.150	.83	79
Son Like Mother vs Son Like Father	6.52	.146	.44	66

In the third procedure, where the choice of 'both' and 'neither' are given to the judges, the chances of there being a real difference between the degree of resemblance of records of the two sexes to their parents is reduced even still further. One cannot avoid the conclusion that the results thus far show no definite relationships between parents and offspring (Tables III, IV).

TABLE III

PERCENTAGES OF SONS OR DAUGHTERS RESEMBLING FATHERS OR MOTHERS (AVERAGE OF JUDGES' RATINGS, WITH CHOICE OF 'NEITHER' AND 'BOTH') *

Relationship	N	Percentage of Former Resembling Latter
Son Like Mother	6	26.07
Son Like Father	6.75	29.34
Daughter Like Mother	4.75	23.75
Daughter Like Father	6.75	33.75

* 32.6 percent of the Sons (N = 7.5) and 23.75 percent of the Daughters (N = 4.75) resemble neither parent.

11.95 percent of the Sons (N = 2.75) and 18.75 percent of the Daughters (N = 3.25) resemble both of the parents.

TABLE IV

SIGNIFICANCE OF DIFFERENCE BETWEEN PERCENTAGES OF SONS AND DAUGHTERS RESEMBLING ONE OR THE OTHER PARENT (BASED ON TABLE III)

Relationships Compared (Greater Percent Given First)	Differences in Percent of Resemblance	σ Diff.	$\frac{D}{\sigma \text{ Diff.}}$	Chances
Daughter Like Father vs Son Like Father	4.41	.142	.31	62
Daughter Like Father vs Daughter Like Mother	10.00	.142	.70	76
Son Like Mother vs Daughter Like Mother	2.32	.132	.17	57
Son Like Father vs Son Like Mother	3.27	.132	.24	60

TABLE V

PERCENTAGE OF SONS OR DAUGHTERS RESEMBLING FATHERS OR MOTHERS (BASED ON DAVIS' FOUR CATEGORIES) *

Relationship	N	Percentage of Former in Same Class as Latter
Son in same class as Mother	7	30.43
Son in same class as Father	6	26.08
Daughter in same class as Mother	3	15.00
Daughter in same class as Father	5	25.00

* 21.73 percent of the Sons (N = 5) and 15 percent of the Daughters (N = 3) are in neither of the parents' class.

21.73 percent of the Sons (N = 5) and 45 percent of the Daughters (N = 9) are in both of the parents' class.

In making the analysis on the basis of the percentage of time alpha waves are present, much the same results are found. In this instance as in the judges' ratings, there seems to be a tendency for the daughters' records to fall into the same classification as that of their fathers' more often than into that of their mothers' and for sons' records to fall into the same classification as their mothers' more often than into that of their fathers'. However, here too the differences are too small to be statistically significant (Tables V, VI).

When comparisons are made including those families in which both parents fall into the same classification, the

TABLE VI

SIGNIFICANCE OF DIFFERENCE BETWEEN PERCENTAGES OF SONS AND DAUGHTERS FALLING INTO THE SAME CLASS AS ONE OR THE OTHER PARENT (BASED ON TABLE V)

Relationships Compared (Greater Percent Given First)	Differences in Percent Falling into Same Class	σ Diff.	$\frac{D}{\sigma \text{ Diff.}}$	Chances
Son Like Father vs Daughter Like Father.....	1.08	.134	.07	53
Daughter Like Father vs Daughter Like Mother.....	10.00	.126	.79	79
Son Like Mother vs Daughter Like Mother.....	15.43	.125	1.2	68
Son Like Mother vs Son Like Father.....	4.35	.133	.32	63

TABLE VII

PERCENTAGE OF SONS AND/OR DAUGHTERS RESEMBLING FATHERS AND/OR MOTHERS (BASED ON DAVIS' FOUR CATEGORIES INCLUDING CASES IN WHICH THE CHILD FALLS INTO SAME CLASS AS BOTH PARENTS) *

Relationship	N	Percentage of Former in Same Class as Latter
Sons in same class as Mother.....	12	52.13
Sons in same class as Father.....	12	52.13
Daughters in same class as Mother.....	10	50.00
Daughters in same class as Father.....	12	60.00

* 21.73 percent of the Sons (N = 5) and 15 percent of the Daughters (N = 3) are in neither of the parents' class.

chances of a child falling into the same classification as only one parent is of course further reduced, and no statistically significant differences are found (Tables VII, VIII).

TABLE VIII

SIGNIFICANCE OF DIFFERENCE BETWEEN PERCENTAGES OF SONS AND DAUGHTERS FALLING INTO THE SAME CLASS AS ONE OR THE OTHER PARENT (BASED ON TABLE VII)

Relationships Compared (Greater Percent Given First)	Differences in Percent Falling into Same Class	σ Diff.	$\frac{D}{\sigma \text{ Diff.}}$	Chances
Daughter Like Father vs Son Like Father.....	7.87	.15	.52	70
Daughter Like Father vs Daughter Like Mother.....	10.00	.156	.64	74
Son Like Mother vs Daughter Like Mother.....	2.13	.152	.14	55
Son Like Mother vs Son Like Father.....	0	.147	0	50

CONCLUSIONS

In conclusion it might be stated that while no data which indicated a certain relationship between any members of a family on the basis of their electroencephalographic patterns can be offered, it is justifiable to assume that the resemblance in the patterns, if any exist, are not marked.

The problem of whether or not patterns in brain potentials are inherited is not a closed one. Davis' work would very definitely indicate that a hereditary factor does exist. Investigations of possible relationships between grandparents and grandchildren are indicated.

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